

Psychological Review

EDITED BY

HERBERT S. LANGFELD
PRINCETON UNIVERSITY

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THE PSYCHOLOGICAL REVIEW

RIVAL PRINCIPLES OF CAUSAL EXPLANATION IN PSYCHOLOGY¹

BY H. M. JOHNSON

Tulane University

If psychology is to become a natural science, it will have to formulate and use some set of causal laws that are at least consistent with the causal laws of the other natural sciences. Some authors have insisted that psychology must even use the same causal laws as physics employs. It is questionable whether this is necessary, or possible. Hence, I propose another procedure which seems to merit discussion.

Consider first, a set of possible events, such as P , Q , R , Each of them may be either simple or compound. A compound event is made up of two or more simple events, any of which may temporally precede, follow, or concur with any of the others.

Consider second, a relation c which is presently to be defined. It may or may not connect one event, such as P , with another event, such as Q . It is symbolized by the first letter in the word 'cause,' and it is akin to certain other relations that have been called 'causal,' although it is not identical with any of them. We shall now define the relation c , exhibit its properties, and appraise its explanatory power.

By definition,

$$P c Q = .(PQ')', \quad (1)$$

i.e., to assert that the event P holds the relation c to event Q , is equivalent to asserting that it does not happen that Q fails if P happens, and it does not happen that P happens

¹ Part of this paper was presented at the 34th annual meeting of the Southern Society for Philosophy and Psychology, held at Durham, North Carolina, April 8, 1939.

if Q fails.² In other words, if condition $P \epsilon Q$ is fulfilled so is condition $(PQ)'$, and conversely, if condition $(PQ)'$ is fulfilled, then $P \epsilon Q$. In still other words, if condition $(PQ)' = .(P \epsilon Q)$ holds, then P is sufficient for Q and Q is necessary to P , and conversely, if P is sufficient for Q and Q is necessary to P , then $(PQ)' = .(P \epsilon Q)$. We might call the relation ϵ 'eventual sufficiency' and its converse 'eventual necessity' where it is necessary to distinguish it from other relations and their converses which are said to be otherwise sufficient or necessary.

How does one seek to determine whether two *given* events, P , Q , are connected by the relation ϵ ? One chooses from two procedures, according to the class of events to which P, Q belong.

There is a class of events each member of which stands in a series with some other members, the series being ordered by the relation C . We may call it 'intrinsic sufficiency' and its converse 'intrinsic necessity.' By definition,

$$P C Q = .(PQ)''^o, \quad (1.1)$$

i.e., to assert that P holds the relation C to Q is to assert that it *does* not happen and *cannot* happen³ that Q should fail if P happens, and that it does not happen and cannot happen that P should happen if Q fails. In other words, if the condition $(PQ)''^o$ is fulfilled, so is the condition $P C Q$, and conversely. In still other words, if condition $(PQ)''^o$ is fulfilled, then P is intrinsically sufficient for Q and Q is intrinsically necessary to P .

² We have replaced certain symbols that are now conventional in modern logic with symbols that have the same meaning as the conventional symbols, and also can be reproduced in this JOURNAL. For example, the conventional sign of negation is a "tilde" prefixed to the symbol of the term to be negated. For it we substitute the symbol (') suffixed to the symbol of the term to be negated. For example: if P denotes "event P happens," then P' denotes "event P fails," *i.e.*, "event P does not happen." Again, if p denotes "proposition p is true," then p' denotes "proposition p is false." The negation of a negation is equivalent, of course, to affirmation; *e.g.*, $(P')' = P$; *i.e.*, the failure of the failure of event P is equivalent to P happening. By defining each unconventional symbol when we first introduce it, and by indicating its meaning now and then thereafter, we hope to minimize confusion.

³ The symbol (") denotes that the expression to which it is suffixed is 'impossible.' See footnote No. 6, p. 498.

One can prove that if P° , i.e., if P cannot happen, then P' , i.e., P does not happen; and conversely, that if P happens, then P is not impossible. The proof, however, is no more convincing than the mere statement that we have just given. We may therefore avoid redundancy by writing P° for P'° , since impossibility implies non-occurrence.

Thus the characteristic of 'impossibility' is more restrictive than the characteristic of 'non-occurrence,' so that some events may not occur, and yet not be intrinsically impossible. Also, the relation C is more stringent than the relation c . If C holds between two given terms, then c also holds; but if c holds, C may or may not hold. Thus, the field of C excludes part of the field of c .

For example, let P_1 be the death of an individual and Q_1 be his birth. Obviously, $(P_1Q_1)^\circ$, i.e., it is impossible that his death should have happened if his birth had not happened, and impossible that he should have failed to be born if he actually died. This is equivalent to his death being intrinsically sufficient for his birth, and his birth intrinsically necessary for his death.

From the foregoing, it is evident that one can determine whether one event P is *intrinsically sufficient* for another event Q , or not, by examining the internal structure of both events. Moreover analysis is the *only* procedure that will yield this determination.

But suppose that analysis does not establish $P C Q$, i.e., that $(PQ)'$ is *impossible*? What can one then do?

One must resort to the second procedure: namely, establish by observation and census whether any exception can be found to the condition $(PQ)'$, i.e., seek diligently for at least one instance in which P happens and Q fails. If the census discloses a single instance of (PQ) , i.e., of P happening and Q failing, then $(P c Q)'$: P does *not* hold the relation c to Q . Moreover, if any exception is found, then $(P C Q)^\circ$, i.e., P *cannot* hold the relation C to Q , for if it did then something which is impossible has nevertheless happened: namely, that $(PQ)'$ and (PQ) , i.e., that Q has failed and P happened and also failed.

Suppose, however, that the census discloses no exception to the summarizing assertion $(PQ)'$, i.e., that Q does not fail if P happens and P does not happen if Q fails. We can then say that all the facts are as they would be if $P \subset Q$, i.e., if P holds the relation \subset to Q . Does this establish that *without exception*, $(PQ)'$? Yes, but on one condition only. If the experience is closed, and the census complete, so that not only have we found no exception among the instances of P, Q that we have examined, but so also that we have examined all the instances there are, then we can assert categorically $P \subset Q$. For example, suppose that the constitution of the United States should be abolished today. Then, we shall have had exactly 31 presidents under this constitution. Let us now ask whether any of them was divorced during his term of office. The true answer turns out to be no. Let P_2 be *becoming president*, and Q_2 be *remaining undivorced*. Observation and counting show that without exception, $P_2 \subset Q_2$, i.e., that if anyone became president, he remained undivorced (either by remaining married, becoming widowed by death, or by remaining single). Becoming president was sufficient (though not *intrinsically* sufficient) for remaining undivorced; remaining undivorced was necessary (though not *intrinsically* necessary) to becoming president.

But suppose that the experience has not yet ended, so that a complete census *cannot* be made, or that for any reason whatever, a complete census *has not* been made. Then, one may indeed say that *within the range of experience*, it has always happened that $(PQ)'$; and that *if* the complete census should yield results exactly like the results of the incomplete census, than P holds the relation \subset to Q .

The procedure that we last described has been called 'induction.' It is to be distinguished from the procedure that makes use of a complete census. At most, induction can assure us that $(P \subset Q)$ is an *hypothesis* which we *may* yet entertain, but which we *must* reject if ever a falsifying instance is found. And unless analysis proves that the exceptional instance is *intrinsically* impossible, we should act as if it *might* be found.

Thus, we have two kinds of procedure: namely, analysis, and observation and counting. By analysis we can show that $P \subset Q$, if we can show that (PQ') is impossible. By observation and counting we can show that *certainly*, $P \subset Q$, if the experience is closed and the census complete. Otherwise, observation and counting can establish some finite *probability* that $P \subset Q$, but cannot establish it with certainty.

Consider next, the relationship of material implication in two-valued logic. If p, q, r, \dots be *any* propositions (whether simple or compound), and i the relation of material implication,⁴ then by definition,

$$p \ i \ q = .(pq')', \quad (2)$$

i.e., if proposition p materially implies proposition q , it is false that q is false if p is true, and false that p is true if q is false; and conversely, if $(pq')'$ holds good, so does $(p \ i \ q)$. In other words, if condition $(pq')'$ is fulfilled, then p is sufficient for q and q is necessary to p .

Note that definitions (1) and (2) have the same form.

Let us now assign specific content to propositions p, q, r, \dots . Let proposition p assert that event P happens; let proposition q assert that event Q happens; and so on. Then, for example, if event P happens, proposition p is true; if event P fails, proposition p is false; and similarly for the other couples $q, Q; r, R$; etc.

Thus we have established a one-one correspondence between the class of possible events and the class of propositions that assert them. Each event corresponds to exactly one proposition; the happening of the event corresponds to the truth of the corresponding proposition; the failure of the event corresponds to the falsity of the corresponding proposition; the relation \subset corresponds to the relation i . What of it?

Something important! For, whenever one class of terms stands in a one-one correspondence with another class of terms, term for term and relation for relation, then every assertion about the interrelations of the terms in one class

⁴The conventional symbol for material implication \supset is a hook, the open end of which lies to the left. Since we cannot reproduce it, we use i instead.

can be 'transformed,' or *translated* into a corresponding assertion about the interrelations of the terms in the other class. And the *proof* of any proposition in the one system can be transformed into a proof of the corresponding proposition in the other system.

By this procedure, Descartes (6) set up a one-one correspondence between an ordered series of numbers and the points in a line-segment, demonstrated a set of propositions about the ordered numbers, and transformed them into propositions about the points in a line. Likewise, he set up a one-one correspondence between the class of ordered couples and the class of points in a plane. Having proved a set of propositions about ordered couples, he transformed them into propositions about points in a plane, and so built up a plane geometry in which drawings to illustrate ideal constructions are unnecessary.⁵

In the present instance we are able to make good use of this procedure. For, the logicians have most obligingly worked out many theorems about propositions connected by the relation of material implication *i*, and we can transform them into propositions about events connected by the relation *c*. The rule of transformation is this: given any theorem that has been proved in the doctrine of material implication, for each symbol denoting a proposition put the symbol for the event which the proposition asserts; for the symbol of *truth* put the symbol of '*happening*'; for the symbol of *falsity* put the symbol of *failure* (i.e., of non-happening, non-occurrence); for the symbol *i* of material implication put the symbol *c* of eventual sufficiency. Thus, one formulates a theorem about events, which is rigidly proved by this procedure, and which can be rigidly proved independently of this procedure in a doctrine of eventual sufficiency.⁶

⁵ For an excellent non-technical exposition of doctrinal function, transformation, and invariance, on which this procedure depends, see Keyser (II, 1-85; 180-189).

⁶ A one-one correspondence exists between the class of events that are interconnected by the relation *C* of intrinsic sufficiency, and the propositions asserting these events, the latter being connected by the relation *I* of *necessary* implication. (This relation is usually symbolized by an eyeless and barbless anchor, lying with its handle turned to the left.)

Thus, our definition $PCQ = (PQ)^{\circ}$ is a transform of the definition of strict

Inasmuch as many readers of this JOURNAL may be better acquainted with events than with assertions about them, I shall not list here all the transformations of theorems of material implication into theorems of eventual sufficiency. Anyone can write out the whole double catalog, or dictionary, in perhaps half an hour. I shall, however, mention, and when necessary indicate the independent development of a few theorems of eventual sufficiency, and merely indicate the corresponding theorems of material implication. Most of the interesting theorems of material implication that would be transformed are shown by Lewis and Langford (13) or by Whitehead and Russell (28). Hereafter we shall designate Lewis and Langford's theorems by a simple number (e.g., 3.7) and Whitehead and Russell's theorems (or propositions) by numbers prefixed by asterisks (*). Some assertions about the relation c that interest us do not correspond to any formally developed theorems in either of these works, although it is obvious that the theorems can be proved.

(I) First of all, the relation c is *transitive*; i.e., if it holds between P and Q , and also holds between Q and R , then it holds between P and R . In symbols: $(P c Q)(Q c R) \cdot i. (P c R)$. For by definition $Q c R$ is equivalent to $(QR)'$, i.e., if R fails Q does not happen; but this outcome is logically inconsistent with $P c Q = (PQ)'$, i.e., if P happens Q happens.

Thus, since $(P c Q)(Q c R) \cdot i. (P c R)$, it is evident that P is sufficient for all events for which Q is sufficient, and that R is necessary to all events to which Q is necessary.

The corresponding assertion in the doctrine of material implication is

$$(p i q)(q i r) \cdot i. (p i r), \quad (3.7), (*3.31)$$

i.e., "the relation of material implication is transitive."

implication $p I q = (pq)^\circ$, which is read variously thus: proposition p strictly implies proposition q ; proposition q is deducible from proposition p ; it is inconceivable (i.e., logically impossible) that proposition q be false if proposition p is true, and inconceivable that proposition p be true if proposition q is false.

The symbol ($^\circ$) suffixed to the symbol of an event means *intrinsically impossible*. If it is suffixed to the symbol of a proposition asserting the event it means *logically impossible, inconceivable*. The characteristics *intrinsically impossible* and *logically impossible* stand in one-one correspondence, but they are not identical. The meaning of ($^\circ$) can always be determined from its context.

(II) Second, the relation c is not symmetrical, and is not asymmetrical. It is therefore called non-symmetrical. Let P_3 be the beheading of an individual and Q_3 be his death. Obviously, $P_3 c Q_3$ but $(Q_3 c P_3)'$. If he is beheaded, he dies, but if he dies it is not necessary that he should have been beheaded. Hence, there are instances in which the relation c is not reversible.

On the other hand, there are instances in which the relation c is reversible. For example, consider two beams of light, one being fixed, the other rotating across it. Consider as events the generation of the four angles of intersection, denoted in counter-clockwise order by A, B, C, D . Let event P_4 be the generation of $A = 30^\circ$; let event Q_4 be the generation, jointly, of $B = 150^\circ, C = 30^\circ, D = 150^\circ$. Then, if P_4 happens, Q_4 happens, and if Q_4 happens, P_4 happens also. In symbols, therefore, $(P_4 c Q_4)(Q_4 c P_4)$. Thus, the relation c is not asymmetrical.

By definition of symmetry, if a relation is reversible between some terms and not between others, it is non-symmetrical. Hence, c is non-symmetrical.

By substituting symbols in the foregoing, one can show that the relation of material implication i is also non-symmetrical. If it were symmetrical, then it would be universally valid to affirm the consequent; if it were asymmetrical, then it would not be reversible in the special instance in which $p = q$. If this theorem is not explicitly developed in a doctrine of material implication, the reason probably is that the author did not need to use it.

(III) Third, in the special instance in which $(P c Q)(Q c P)$, we assert as a primitive proposition that P and Q are *equivalent*. This does not mean that the two events are *identical*, but that if either happens, the other happens also. Hence, each of them is sufficient for all events for which the other is sufficient, and necessary to all events to which the other is necessary.

The corresponding assertion in the doctrine of material implication is

$$(p i q)(q i p) = .(p = q). \quad (2.2; *4.01)$$

In Lewis and Langford's development this assertion is a theorem; in Whitehead and Russell, a definition.

(IV) Fourth, the relation c is *reflexive*, i.e., $P c P$; P holds the relation c to *itself*. For, in the defining equation (1), P and Q are severally *any* possible events. We may therefore substitute P for Q , and so get

$$P c P = .(PP)',$$

i.e., if P holds the relation P to itself, then it does not happen that P fails if P happens, and it does not happen that P happens if P fails. By corresponding substitution in definition (1.1) we could have shown that $P C P = .(PP)^\circ$, i.e., that a possible event *cannot* fail if it happens, and it cannot happen if it also fails.

In material implication, the corresponding assertion is

$$p \dot{\supset} p = .(pp)' \quad (2.3; *2.08)$$

and in strict implication the corresponding assertion is

$$p I p = .(pp)^\circ. \quad (12.1)$$

The right member of these equivalences states the law of contradiction.

The condition $(P c P)$ may be interpreted thus: Every event is both necessary and sufficient for itself; and $(P C P)$ may be interpreted: Every event is intrinsically necessary and intrinsically sufficient for itself. One might therefore ask, "Then why look for other antecedents?" We shall indicate a suitable answer to this question a little later.

(V) Fifth, the relation c is *many-many*, in the sense that 'many' means 'one, or more than one.' First, a given consequent may have many interchangeable antecedents. For example, let Q_6 be the compound event 'death.' Let P_6 be drowning, P_7 freezing, P_8 beheading, etc. Observation establishes, within the uncertainty of incomplete enumeration, that $P_6 c Q_6$, i.e., that it does not happen that death fails if drowning happens; $P_7 c Q_6$, i.e., that it does not happen that death fails if freezing happens; $P_8 c Q_6$, i.e., that it does not happen that death fails if beheading happens; etc. Drowning,

freezing, beheading, each is sufficient for death, death is necessary to each.

In judging whether two or more antecedents are equivalent in the sense that they severally hold the relation c to a given consequent, we have to define the consequent unequivocally. In the instance that we have just considered, P_6 , P_7 , P_8 , each has other consequents besides Q_6 , death. If event R_6 is flooding the lungs with water, event S_6 the bursting of cell-membranes from over-expansion, event T_6 the replacement of arterial blood with air, then we get $P_6 c Q_6 R_6$, *i.e.*, drowning is sufficient for both death and flooding; $P_7 c Q_6 S_6$, *i.e.*, freezing is sufficient for both death and bursting; $P_8 c Q_6 T_6$, *i.e.*, beheading is sufficient for both death and replacement; etc. Thus each of these antecedents has a different set of consequents from each other antecedent, although one consequent, namely Q_6 , death, is a member of each set. Thus, P_6 , P_7 , P_8 , are mutually equivalent in the sense that each is sufficient for death, but they are not mutually equivalent in the sense that each is sufficient for the same set of joint consequents. Is it generally true that the joint consequents of each antecedent form a unique set?

Wolf (29) says that it is. He asserts that if one completely described every antecedent, and also every consequent, one would find that each antecedent had one and only one set of consequents, and that each set of consequents had one and only one antecedent. But although his assertion looks plausible, it cannot be verified. It cannot be verified by observation and counting, for experience stands open, and the census is incomplete. It cannot be verified by analysis, for analysis shows the opposite is true.

For example, the operation of cubing a number is an event. Let P_9 be the cubing of 1; P_{10} the cubing of $(-\frac{1}{2} + \frac{1}{2}\sqrt{-3})$; P_{11} the cubing of $(-\frac{1}{2} - \frac{1}{2}\sqrt{-3})$. Then $P_9 c 1$; $P_{10} c 1$; $P_{11} c 1$. Each of the antecedent events has the same consequent: namely, the number 1; but they are not identical.

Again, the extraction of the cube root of a specified number is an event. Let P_{12} be the extraction of the cube root of 1. Let Q_7 be 1; Q_8 be $(-\frac{1}{2} + \frac{1}{2}\sqrt{-3})$; Q_9 be $(-\frac{1}{2} - \frac{1}{2}\sqrt{-3})$.

Then $P_{12} \text{ c } Q_7$; $P_{12} \text{ c } Q_8$; $P_{12} \text{ c } Q_9$. Thus, one antecedent P_{12} has many consequents Q_7, Q_8, Q_9 , and the consequents are not mutually equivalent in certain other respects.

Similarly, consider a 'point-source' of radiant energy, emitting in *quanta*, and being located at the center of a sphere, the interior surfaces of which are made up of elements equal to each other in area. According to theory, the source will radiate energy at the same time-rate to each of the receiving elements and the results of experiment agree with theory within the uncertainty of measurement and the uncertainty of approximation of actual conditions to theoretical conditions. According to theory, if the time-rate of emission is great enough and the time T during which emission occurs is long enough, then within the duration T , each element of the receiving surface will intercept as many *quanta* as any other. But, suppose we reduce the rate of emission, or shorten the duration of emission, until within the time T only one *quantum* is emitted. The *quantum* being, by hypothesis, indivisible, it will be intercepted by at least one and at most one of the receiving elements. Call the emission of one *quantum* event P_{13} ; call its interception Q_a, Q_b, Q_c, \dots , according as it is intercepted by element a, b, c, \dots of the receiving surface. Then we have

$$P_{13} \text{ c } Q_a \text{ or } Q_b \text{ or } Q_c \text{ or } \dots,$$

but given the single antecedent event P_{13} we cannot tell *which* of the alternative consequents will happen, although we know that at least one and at most one of them will happen. This situation is by no means non-typical. *It remains to be shown* that P_{13} has not been exhaustively analyzed, and that Q_a, Q_b, Q_c , have each a unique cause. Meanwhile, the hypothesis $P_{13} \text{ c } Q_a \text{ or } Q_b \text{ or } Q_c, \dots$ is consistent with all the facts of observation.

That the relation of material implication i is also many-many is easily shown. For, in the defining equation (2) the terms p, q , denote severally *any* propositions. Any given proposition may be either simple or compound: hence p may assert " a is true or b is true," or " a is true and b is true, . . ."

while q may assert " w is true or x is true" or " w is true and x is true, . . .". Hence, either p or q or both p and q may be either simple or compound.

The corresponding propositions

$$(q \text{ i } r) . i . (p \text{ or } q . i . r)$$

and

$$(q \text{ i } r) . i . (pq . i . r)$$

are easily proved in the doctrine of material implication. Likewise,

$$(q \text{ I } r) . I . (p \text{ or } q . I . r)$$

and

$$(q \text{ I } r) . I . (pq . I . r)$$

are easily proved in the doctrine of strict implication.

The doctrine of material implication contains two theorems that stand in the relation c to many headaches. The doctrine of strict implication contains two corresponding theorems.

In material implication, one of these cephalgic theorems is

$$p' . i . p \text{ i } q, \quad (7.41; *2.21)$$

i.e., if a proposition is false, then it materially implies any proposition. But if this is so, then not only

$$p' . i . p \text{ i } q,$$

but also

$$p' . i . p \text{ i } q',$$

whence, by the operation of adjunction,

$$p' . i : p . i . q \text{ or } q'.$$

Thus if p is false, the truth of q does not depend on the truth of p . In fact q is *deducible* from p only if

$$p . p \text{ i } q,$$

i.e., if p is true and if also p materially implies q .

The corresponding assertion in strict implication is

$$p^\circ . I . p \text{ I } q, \quad (19.74)$$

i.e., if a proposition p is self-contradictory or impossible, then

any proposition q is deducible from it. In other words, *if* any proposition is both self-contradictory and true, then it is inconceivable that any proposition should be false. This equivalence restates the law of contradiction.⁷

(VI) The corresponding assertion about the relation c is

$$P'.c.PcQ$$

and by properly substituting symbols in the doctrine of material implication we get

$$P'.c : P.c.(Q \text{ or } Q'),$$

i.e., granted that P fails, then if P nevertheless happens, Q either happens or fails. But although the right member of this assertion has the same *form* as (I), nevertheless it is trivial. For one event to insure a second event,

$$P.PcQ,$$

i.e., not only must PcQ but also P must happen.

In a doctrine of intrinsic sufficiency, the corresponding assertion is

$$P^{\circ}.C.PCQ,$$

given an event that cannot happen, if it nevertheless happens then every event happens.

In material implication, the second cephalgic theorem is

$$q.i.piq, \quad (7.4; *2.02)$$

i.e., if any proposition q is true, then it is false that q is false and any other proposition p true.

⁷ For example, let proposition p assert jointly three sentences from the Westminster Confession: namely (1) that God is the creator of all things both visible and invisible, and hath foreordained, for his own glory, whatsoever cometh to pass; (2) that God is not the author of evil; (3) that, nevertheless, some actual events are evil. It has been argued that these three assertions are not mutually compatible, so that if p asserts them jointly, then p is impossible; whence, if p is true, no proposition can be false. In general terms, *if* the inconceivable is true and therefore conceivable, then nothing is false.

Lewis and Langford dispose of this paradox and its brother in material implication by asking (13, p. 175) when we wish to draw inferences from a self-contradictory proposition.

Its correspondent in strict implication is

$$q.I.p I q = (q')^{\circ}.I.p I q, \quad (19.75)$$

i.e., if the contradictory of a proposition is inconceivable, then it is inconceivable that the proposition be false and any other proposition true.

(VII) The correspondent of (7.4 and *2.02) in our system is

$$Q.c.P c Q.$$

If event Q happens, then any event is sufficient for it, and it is necessary to every event, *i.e.*, if Q happens, then it does not happen that Q fails and any other event P happens. This reduces to the assertion: If Q happens, then Q happens.

The correspondent of (19.75) in our system is

$$Q.C.P C Q,$$

which reduces to $Q.C.(PQ')^{\circ}$, *i.e.*, if Q necessarily happens, Q cannot fail if P happens. And this reduces to the assertion $(QQ')^{\circ}$, it is impossible that Q should both fail and happen.

These seven theorems are the only ones that we shall discuss here.

Suppose, now, that an event Q happens: How shall we 'explain' it? By (IV), we may set up hypothesis H_1 , namely that $Q c Q$, *i.e.*, Q is self-sufficient. But by (VII) we may set up hypothesis H_2 , namely that *any* event is sufficient for Q , in the sense that if Q happens, then $(PQ')'$, *i.e.*, Q does not fail whatever happens. Also, by (V) we may hypothesize that *some* particular event is sufficient for Q , that event remaining to be determined certainly by analysis or by a complete census of a closed experience, or to be determined probably by observation and induction.

Now it so happens that it is impossible to falsify either H_1 or H_2 . If we accept either (or both) of them, as a complete answer, we look no farther for an 'explanation' of Q . But they are compatible not only with each other, but also with H_3 , namely, that . . . or P_k or P_i or P_j or . . . is sufficient for Q . We can test these alternatives separately, noticing

whether any of them can happen without Q happening. If we find exceptional instances, namely of Q failing even though some P happens, we can eliminate *that* P . But suppose we have left a class of P -events, P_i, P_j, P_k, \dots , such that if Q fails, then P_i fails and P_j fails and P_k fails and \dots fails, and if P_i happens Q happens, or if P_j happens Q happens, or if P_k happens Q happens, or if \dots happens Q happens. In other words, Q is necessary to *each* of these P -events, while *any* of these P -events is sufficient for Q . What then?

Well, any of these P -events may be either simple or compound. If we find it to be compound, we undertake to analyze it, and by eliminating its constituents one after another, we *try* to decide whether the remaining constituents are jointly sufficient for Q , and individually necessary to Q . But we are embarrassed by two properties of the relation c , which belong to the relation C also, and indeed, to every causal relation that presupposes c .

First, by (I), the relation c is transitive. Hence, if $P c Q$, then every antecedent of P also holds the relation c to Q . Second, by (IV), the relation c is many-many. Hence other events may be interchangeable with P in the relationship $P c Q$. One can find no stronger instance than one that satisfies the condition

$$(P C Q)(Q C P) = (PQ')^{\circ}(QP')^{\circ},$$

i.e., the condition that neither event *can* fail if the other event happens, or happen if the other event fails; in other words P is *intrinsically* necessary and sufficient for Q , and Q is *intrinsically* necessary and sufficient for P . Nevertheless, this condition does not exclude other events from being interchangeable with P in the relationship, for it can be readily proved that the relation C is transitive and many-many. Hence, we cannot demonstrate that there is any event which is *uniquely* necessary and sufficient for any other.

From these considerations, it seems to me that Claude Bernard's aim, as described by Msgr. O'Toole (18), is impossible of realization. For Bernard sought to identify all the conditions that are individually necessary to an event,

whereas the relation of necessity \tilde{c} like its converse relation c is both multiple and transitive. Hence, observational procedure *cannot* determine a set of events that is *uniquely* necessary to event Q , or even prove that such a unique set exists. Moreover, as O'Toole points out, even if one could identify all the events that are individually necessary to Q , one thus derives no insurance that Q must happen, or even that Q will happen. To deduce from these premises that Q must or will happen would be to affirm the consequent. To draw a valid conclusion, one needs another postulate: namely, that there is an order in Nature such that any event Q will happen *unless* some other set of events fails. This postulate is *consistent with* Malebranche's doctrine of Occasionalism (16), and also with Spaulding's doctrine of Contingency (26), which might be regarded as a godless Occasionalism. But to guarantee this postulate, one must transcend observational experience, and perhaps even contradict observational experience.

It may seem that the relation c lacks explanatory power. Indeed, it does not yield all that one might desire of a causal relation. Nevertheless, unless one can truly assert $P c Q$, one cannot truly assert that *any* causal relation connects these two events. For if we find a single instance in which (PQ') , *i.e.*, in which P happens and Q fails, we must deny (a) that P is sufficient for Q ; (b) that P is intrinsically sufficient for Q ; (c) that P is both necessary and sufficient for Q ; (d) that P invariably precedes, follows, or concurs with Q ; (e) that God has ordained that if P happens, Q will not fail; (f) that the 'perfect man' will be convinced that P causes Q ; etc. Each of these propositions asserts *some* causal relation to hold between P and Q , but if (PQ') , each of them is false.

One can show, by enumeration, that all other relations that are usually called 'causal' are compounded of the relation c and some other relations in a logical product. Consider for example assertion (d). Let b , which is the first letter of the word 'before,' denote the relation 'temporally precedes.' Some authors say that if one uses language 'properly'—*i.e.*, according to their taste—then to assert " P causes Q "

is equivalent to asserting $(P \text{ } b \text{ } Q)(P \text{ } c \text{ } Q) = .P \text{ } bc \text{ } Q$, i.e., that " P precedes Q and is sufficient for Q ." Others are more stringent. They desire the expression " P causes Q " to be equivalent to $(P \text{ } b \text{ } Q)(P \text{ } C \text{ } Q) = .P \text{ } bC \text{ } Q$, i.e., " P precedes Q and is intrinsically sufficient for Q ," or, in other words, " P precedes Q and also necessitates Q ." Still others say that the assertion " P causes Q " ought to be equivalent to $(P \text{ } b \text{ } Q)(P = Q)$, i.e., " P precedes Q and is both necessary and sufficient for Q " in the sense that P precedes Q and also that Q does not fail if P happens and does not happen if P fails. One distinguished philosopher recently said that he would be satisfied if the assertion " P causes Q " was made equivalent to the assertion that " P invariably precedes Q and some person *feels* that the temporal order is necessary." (According to this definition, would the sun's radiation have 'caused' the earth's surface to become warmer until man appeared to make the judgment?)

Notice that the relation c is a logical factor in each of these compound relations. It is therefore necessary though not sufficient for each of them. But each of them is attended by all the limitations that belong to c , as well as by others. Nevertheless I assert that the other components in these different causal relations cannot be established by observation in a system that is continuing; and I suggest that the relation c establishes *an* order within a system of events such as to satisfy all the needs of a mere scientist.

Moreover, to insist that a causal relation must be compounded of other factors besides the relation c is to insist that we replace a relation that we use in both science and daily life with a relation that we use in neither. Consider, for example, the relation 'invariably precedes.' It is not implied in the relation c , for c is independent of temporal order. So, I believe, is any causal relation that works out in practice. For, *if* the world of perceptual objects and their interrelations is 'well ordered,' then every event of this instant is correlated with every other event of the same instant, and also with events of the past and events of the future. Granted, that some portion of the perceptual world is not 'well ordered,'

it follows that that portion does not lend itself to scientific description and explanation. Therefore, science must limit itself to the portion that is well ordered, if such there be. But within that portion, we find three causality-principles available, according as the antecedent event temporally precedes, follows, or concurs with the consequent event. Each of these causality-principles presupposes the relation *c*. We call them respectively (a) *mnemic causation*, (b) *telic causation*, (c) *efficient causation*. Let us look at (c) first.

When we use *efficient* causation, we seek an explanation of the events at one instant by examining other events that occur within the same instant. Ordinarily, though not necessarily, we describe these relationships in terms of energy-transformations and equivalences. Thus, we explain the changes in the directed velocities of two colliding billiard balls by mentioning their sizes, masses, original directed velocities, elasticities, coefficients of friction between ball and table and between ball and air, etc. These relationships may be expressed in the form of differential equations, which tell us how the distribution of energies within the system is changing *at an instant*, but which tell us nothing about the condition of the system in the past or in the future. The laws of efficient causation are most used in the physical sciences, in which many of them have been conveniently formulated in terms of energetics. It is often asserted that the physical sciences make use of no principles of explanation except laws of efficient causation, but as we shall mention later, this assertion is false.

In *mnemic* causation we seek an explanation of the behavior of a system at one instant by taking account of the events in its past (19, 21, 24, 25). For example, we say that the child now dreads the fire because some time ago he burnt his fingers in it; that my dog now 'sits up nicely' while the tea and cakes are being passed because last summer a guest fed him when he did so; that my bathroom scale now registers a 2-pound load when its platform is empty because I stood on it too often while I was trying to reduce; that an electromagnet now attracts iron filings while no current is passing through its coil because it was energized ten minutes ago.

In like manner we often seek for an earlier event that is sufficient for a later event.

In *telic* causation we seek an explanation of the events of one instant among the events that come thereafter. Thus the divorce D of a couple occurs after their marriage M ; nevertheless the divorce is sufficient for the marriage: $D \epsilon M$. Indeed, D is intrinsically sufficient for M , for $(DM')^\circ$, i.e., it is impossible that D should occur if M does not occur. I grant that it violates a language habit to say that divorce causes marriage, but the violation is not a violation of principle. For example, some say that Christ died that the scriptures might be fulfilled. If the scriptures are fulfilled, then he died—fulfilment is sufficient for his death, as his death was necessary to fulfilment. At least, so it is said. Similarly, I bought an airplane ticket last Tuesday because I flew to Washington last Wednesday; the flight was sufficient for the purchase; the purchase was necessary to the flight. My office-girl spent four hours and five dollars in a beauty-shop last Saturday afternoon because she was going to a dance with a new beau Saturday evening. My dog has just 'sat up nicely' and barked because he was going to be fed a few seconds later. If I dip a net into soap-suds, I get a film that vacillates until it assumes a certain form *because* when it takes this form it will minimize its boundary-surfaces (15). If it minimizes them, it takes this form: thus, minimization is sufficient for the assumption of the prescribed form, while the form is necessary to minimization.

Two limitations on the uses of *telic* causation should be mentioned. First, as Russell (22) mentioned in his essay entitled "On the Notion of Cause," we cannot explain events of the present in terms of events of the future, because we do not know what the future will be. However, if both events are now past, we can, and often do, explain the earlier in terms of the later, by showing that the later is sufficient for the earlier and the earlier is necessary to the later. Second, when we make use of *telic* principles of explanation, we do not have to assume that the system in its earlier state was 'con-

scious of' what its later state would be. Finality does not presuppose purpose, neither does it exclude purpose.

The three kinds of causality-relations that we have just mentioned are not antagonistic, but supplementary. For example, consider the class of phenomena called hysteresis in non-living physical systems. The iron core of my electromagnet now attracts iron filings because it was recently energized. Its present behavior has been determined in part by its past experience. If I let it rest until tomorrow, it may 'forget' today's experience. The physicist assumes that it behaves as it now does because its structure was changed by its being energized; that more of its molecules are now oriented in one direction than in any other; and that the degree of its present magnetization depends on the present structure, or the present configuration of its fields and forces. Perhaps this is so, and eventually, he may be able to prove it. But in the meantime if he knows the original characteristics of the sample of iron, and knows also when, how often, and how strongly it was energized, he can determine its magnetization at this instant. Again, if a sample of distilled water is superheated and cooled under certain specified conditions, it will thereafter boil at a new critical temperature, which may differ widely from 100°C . Or, if a sample of distilled water is supercooled and then brought to room-temperature under certain prescribed conditions, it will thereafter freeze spontaneously at a new critical temperature, which may be as low as -16°C . (7). The physicist may assume that while the water was undergoing this unusual treatment, its structure was altered, and that its new physical properties are what they are because its structure is what it is. Perhaps he may eventually prove that this is so; but in the meantime, if he knows how the sample has been treated, he can derive its freezing point and its boiling point. Thus can he substitute mnemonic causation for efficient causation (19).

Furthermore, consider the forms of liquids. Immerse a drop of water in an oil, and note the form that it assumes. Why did it take this form? Says Mach (15), 'in order to' minimize its boundary-surfaces. This is telic explanation.

Perhaps an able physicist could refer the phenomenon to a configuration of fields and forces, and so provide an explanation in terms of efficient causation. But both explanations hold true, and we may use the one that is most convenient.

Many psychologists announce in their introductory chapters that they are going to be scientific, and *therefore* they will describe and explain bodily movements according to the causal laws of physics—*i.e.*, in terms of energetics and efficient causation. If a psychology of this kind should be completely worked out, it would be an example of biophysics. But thus far, hardly a beginning has been made. The organs in the reactive mechanism do not lend themselves readily to examination, and the detectors that we now have can tell us very little about the nature of the physico-chemical processes; certainly, they do not tell us enough to enable us to make many predictions. Hence, is it surprising that the authors usually forget their promise? As soon as they begin to discuss memory, association, learning, perception, they begin to use principles—by no means well formulated—of mnemonic causation, substituting biographical information for information about energy-distributions. And if they include a discussion of intelligence and volition—*i.e.*, of the *adaptive* functions of behavior—they are very likely to use telic causation. In using mnemonic and telic explanations, they are, of course, proceeding scientifically; their mistake lay in repudiating these principles at the outset and then using them unsuspectingly. It is the scientist's business to use ordering relations wherever he can discover them; and if he cannot order his facts according to one causality-principle, he should use another. But this is not all. Not only may the psychologist use mnemonic causation and telic causation to account for phenomena that he cannot order by efficient causation: he ought to use mnemonic and telic principles anyway. I dare say that even if he *could* describe the writing of an invitation as a series of energy-transformations, he *would* not do it, for it is the biographical and final relations of this event that most interest him, and these are not readily discernible in efficient

causality explanation, while they are capable of being manifested in the other types of explanation.

We have shown that the relation *c* has the important advantage of being a non-temporal relation, so that it is implied in mnemonic causation, efficient causation, and telic causation. But it has certain other advantages over certain other causality-principles, which we must mention as briefly as we can.

Certain other causal relations can connect only those events that are *of the same kind*. The axiom, "Every cause is similar to its effect," (cf. 17) is in my opinion a very expensive luxury. For example, *if* under certain conditions that one can specify, I should expose my eye to a flame of lithium salt, I would *see red*. Now the relation *c* connects these two events, as well as connecting many temporally intermediate events with the event of seeing red. Among them are (by hypothesis) some chemical interactions in my retina, some detectable electrical disturbances in my nerve-tissue, etc. Now *if* these events occur, *then* I see red; *perhaps* if some of them are lacking, I do not see red. If we call the relation *c* a causality-relation, then we can say that this class of physical events—namely movements of material particles, if you like—*causes* me to see red. But if we admit the assertion "every cause is similar to its effect" as an axiom, then we cannot say that this train of physical events causes me to see red. For the act of perceiving is a mental event; it may indeed be correlated with the movement of material particles within my reaction-arcs; but it in no wise resembles such movement. But in practice we say that response and cognition are causally interrelated; that the stimulus-response series is sufficient for perception, and on the other hand that perception may be sufficient for an ensuing response. The assertion is meaningful, and it may even be true, but it is inconsistent with the axiom of similarity. I prefer to reject this axiom rather than deny a causal relationship. And the only causal relation that it seems possible to establish as holding between physical events and mental events is the if-then relation *c*.

Finally, the relation c may be usefully employed in instances which contradict the axiom "Every cause is quantitatively equivalent to its effects." (Cf. 2, 28.) This axiom might hold in efficient causation, if both event-terms are evaluated in energy-units, and if the whole situation is considered as it stood 'at an instant.' But obviously we contradict the axiom if we assert that perception 'causes' movement, or that movement 'causes' perception, for movement and perception have no common measure.

In view of all that we have been considering, I suggest that psychologists should regard the relation c as being not only 'a' causal relation, but also as being adequate for explanation of most, and perhaps all the facts with which they deal. Whenever they can truly assert 'If P then Q ,' or 'If Q then P ,' they may as well assert ' P causes Q .' Since they can assert $P c Q$ without regard to temporal order, let them use efficient causation where it is convenient, mnemonic causation where it is convenient, telic causation where it is convenient, and any two or all three kinds of causation where it is convenient to use them.

To follow this suggestion, one needs to have all three kinds of causal laws detected and formulated! Thus far we have made but little progress in detecting and formulating laws of mnemonic causation and telic causation. Perhaps the reason is that we have disparaged them for fear of looking unscientific. Why should we not seek to establish such laws, and then employ them premeditatedly, and without shame?⁸

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LEWIN'S 'TOPOLOGICAL' PSYCHOLOGY: AN EVALUATION

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Perhaps the first reaction of most psychologists to the title of Professor Lewin's book—*Principles of Topological Psychology*—is a feeling of dismay that still another variety of psychology must be added to an already over-long list. Furthermore, and contributing to our psychologist's disgruntlement will doubtless be his ignorance of the meaning of topological, albeit he should waste little time in self-reproach on that count. As a relatively new branch of mathematics, topology is not well known even to American mathematicians; and hence it could hardly be expected to strike a familiar note to the ears of American psychologists.

In a non-technical way, topological psychology may be described as an attempt to comprehend human behavior in terms of the objects which are present, and of the relations among the 'events' which are taking place, in a given environment ('life-space'). Perhaps the best way to show topology in action is to record instances in which it has been applied. The following are taken from Lewin (4, p. 47, pp. 136-137). A mother takes her one-year-old child away from his play in order to feed him on her lap. The child does not want to eat and struggles to free himself from his mother's restraining arm. He wishes to play rather than eat. Now represented topologically, the child appears as a small circle in the given plane; his spoon is indicated by a small rectangle. A heavy line or barrier (set up by the mother's restraint) surrounds the child and marks off his 'region of eating' from the larger 'region of play.' The 'region of eating' is roughly an ellipse, the 'region of play' a larger ellipse partly overlaid by the 'region of eating'. . . . A man is in prison. Topologically, the

walls of the prison are represented by a circle (a 'Jordan curve'). Within the prison circle are smaller circles or 'barriers' until the cell (a very small circle) which contains the prisoner (denoted by the letter 'P') is reached.

I suspect that these examples will strike most readers as novel but somewhat cumbersome portrayals of fairly simple psychological situations. Are they anything more than this? Most of the psychologists whose opinion I have sought think not. Lewin, on the other hand, in *Topological Psychology* and in its earlier companion volume (3) contends that topology provides the basis for a truly scientific psychology. It is this claim that I propose to examine here. There are at least two ways, it seems to me, in which topology might represent an advance in our comprehension of psychological data. It might enable us (1) the better to describe behavior in a given environmental setting; and it might (2) provide clearer insight into motives underlying observed behavior. Let us consider these two propositions in order.

The contention that Lewin's diagrams tell us nothing that we could not get from a careful verbal account is an obvious objection, and would, I am sure, be regarded by Lewin as quite superficial. Edna Heidebreder in a highly appreciative review (2) of Lewin's *Topological Psychology* has anticipated this criticism and has answered it more succinctly—if not more convincingly—than has Lewin. After protesting that topological diagrams are 'not in any sense given as visual pictures' of the 'psychological situation,' she writes (p. 588): "Anyone misses the point entirely who regards the diagrams and figures with which the conceptions are represented as more or less than means of indicating the topological structure which represent psychological facts conceptually. The diagrams and figures themselves are of secondary importance, and are even misleading if interpreted in terms of the usual metrical geometry. They represent the situation not perceptually but conceptually. They neither are nor pretend to be pictorially faithful to the facts; they represent mathematical concepts, and it is the special characteristic of mathematical concepts as distinguished from other symbols, such as

those of ordinary speech, that they belong to a system of concepts which are related to each other in a univocal way." It is clear, at least, from Dr. Heidebreder's discussion that we are not to take topological diagrams as accurate geometry. And I suppose we should all agree with her that mathematical concepts (*e.g.*, point, line, plane, solid) provide an especially concise system of logically related ideas. But it is not at all clear to me how or why mathematical symbols invest behavior data with their peculiar 'univocal' character. Do the psychological facts or the relations observed take on some remarkable new 'conceptual' character as a result of topological representation? By means of mathematical terms and mathematical relations one can, to be sure, translate the observed behavior facts into spatial and pseudo-dynamic terms. But psychology no more becomes a system of rigidly connected concepts *simply* by the use of topology than a boy becomes a man by putting on his father's hat.

Lewin defends the use of mathematics in psychological analysis on the grounds that behavioral events, being spread out in time and space, especially favor topological representation. He writes (4, p. 54): "It is now generally recognized that the whole-part relationship, and the relationships of the parts to each other play a fundamental role in psychology. This is true for all branches of psychology. The concept of connectedness, for instance the distinction between separate and connected regions, the distinction between different groupings of regions, is as we have shown above of prime importance for characterizing both the person and the psychological environment. . . . One can coordinate certain psychological facts which have the function of a psychological connection between two psychological 'points' to a 'path' which mathematically connects two points. For instance, any kind of locomotion of the person in the quasi-physical, the quasi-social, or the quasi-conceptual field can be designated as a connecting process which corresponds to a topological path. . . ."

It is doubtless true that line sketches, like football diagrams, are more neatly descriptive of behavior activities in a

given environmental setting than are, say, analogies in terms of taste, odor, or color combinations. But the topological diagram can only picture the spatial set-up; it cannot represent the complex interplay of motives. Spatial concepts apply logically when social behavior, for example, involves 'locomotion' or movement in space. But spatial analogies are not helpful when anger, aggressiveness, honesty, and the like, are to be represented. Arrows picturing angry movement in the social field toward or away from some object, circles representing regions of 'free' movement, and lines representing 'barriers' to aggression are so oversimplified as to be almost ridiculous. (It is only fair to add that Lewin has promised a 'vector psychology' to account for psychological 'forces'.)¹ To repeat, is it necessary to employ a host of mathematical and pseudo-mathematical concepts in order to envisage psychological situations? The excellent studies of Jersild and his associates at Teachers College, and the careful analysis of sympathy by L. B. Murphy (6) have managed to illuminate genuine psychological concepts without benefit of spatial analogy. Even Lewin, in discussing the experimental work of his associates, does not always use spatial and 'dynamic' analogies (3, Chap. VIII). The results of the work of Frank on aspiration level, for example, are presented without the aid of topology.

Objection to the notion that topological nomenclature adds some transcendent conceptual quality to the psychological phenomena does not, of course, argue that it is never useful to devise new terms. Our inability or unwillingness to concoct operational definitions for such terms as instinct, personality and the like accounts in large part for the confusion which is present in the use of these concepts. Although Tolman's 'means-ends-readiness' is certainly clumsy (it always reminds me of the phrase 'auf-dem-Kopfstehendes-Bild') it does serve to characterize observed behavior in the

¹ Lewin has recently presented 'hodology' (5) as an extension, and presumably more useful development, of topology. The value of this new 'ology' still lies in the future.

process of learning a maze, and as such is useful. I doubt if the same can be said of such terms as 'action,' 'boundary,' 'life-space,' 'region,' 'field,' 'behavior,' which Lewin considers to be *bona fide* psychological concepts. All of these terms are mathematical or pseudo-mathematical; even 'life-space' is defined as the 'totality of facts which determine the behavior of an individual at a certain moment' while 'behavior' is defined as the 'changes in the life-space.' Again and again Lewin emphasizes the *conceptual* gain to psychology which will result from topological analysis. But as already pointed out, does not the 'scope and unambiguousness of these relationships' which Lewin so highly prizes reside solely in the mathematical nature of the concepts themselves? Whether the underlying *psychological relationships* which are represented by topology are also 'unambiguous' is for experiment, not logic, to determine. When working with mathematical symbols we should guard against the real danger that one may so forget his primary psychological interest that—like the grin of the Cheshire cat—only the mathematics remains.

In attacking the question of 'causation,' it should be noted that Lewin believes that adequate description constitutes explanation. He writes (4, p. 82): "What we are trying to do is to represent situations in such a way that the events follow from them 'self-evidently,' namely as purely logical consequences. If one wants to call this 'description,' it is not worth while to quibble over words. But if one considers conceptual derivation and the transition from phenomenal to dynamic facts as the characteristics of an explanation, then what we have here is in fact explanation." The attempt to seek an explanation 'behind the facts' rather than in terms of the dynamic relations present in the situation is for Lewin a return to 'Aristotelian thought.' Aristotelian thought is carefully to be distinguished from 'Galilean thought.' The former, according to Lewin, extrapolates from fundamental principles, and looks for final causes, while the latter is concerned with immediate or 'systematic' causes within the life-space. Lewin's distinction between ancient and modern

attitudes toward causation, proof, and scientific method is clearly drawn, and his examples taken from physics are enlightening. Application of the same notions regarding causation to psychology, however, would certainly restrict our descriptions of a psychological event. Thus, since topological diagrams deal with systematic causes within the environment, they inevitably have a 'here-and-now' quality which limits their usefulness. One notes that in his outline of experimental studies of tensions (3, Chap. VIII) Lewin's emphasis upon immediate causes leads him to minimize background (*i.e.*, 'historical') factors which are often important.

One may agree with Lewin's distinction between Aristotelian and Galilean modes of thought without accepting his characterization of many present-day psychological concepts as Aristotelian. Let us consider two very different examples. Lewin cites as Aristotelian the psychologist's use of the term 'drive,' and of the term 'average,' the latter to the neglect (as he sees it) of the individual. But is 'drive' truly an Aristotelian concept? I think not. This term fulfills neatly the definition of a concept: It describes a condition common to many behaviors, and can be identified in a variety of psychological events. There is no more need for the psychologist to think of drive as a force or 'demon' than there is need for the physicist to think of gravity as a thing-in-itself. As for criticism of the use of the term average, since the psychologist must work with a scale the units of which are usually unequal and the zero point of which is certainly unknown, he must perforce employ the average or mean as a datum point from which to describe the individual. Lewin thinks that the psychologist forgets the individual to render homage to a mathematical point which describes a hypothetical person who does not actually exist. But this is not true. The curve of development of any given child—his I.Q., height, or knowledge of history—takes on meaning only when referred to some norm, say the mean performance of his age group. A single child may illustrate a 'law of development' but he cannot possibly establish one. A separate law for each person is

like a unique trait. The person who possesses a unique trait cannot know that he has it; and no observer can possibly recognize it.

Several minor criticisms of topology as a means of representing psychological 'causes' may be noted in passing. Based upon the distinction between Aristotelian and Galilean modes of thought, Lewin makes a division between the phenotype and genotype or between 'descriptive' and 'conditional-genetic' concepts. An illustration from physics (3, p. 11) will make the difference between the two terms clear. The orbits of the planets, the free falling of a stone, the movement of a body on an inclined plane, the oscillation of a pendulum, which if classified according to their phenotypes would fall into quite different classes, are all expressions of the same fundamental law, have the same genotype. This distinction, so clear in physics, is not always adhered to in the descriptions which Lewin gives of the work of his students. In the experiments of Karsten, Fajans, and Dembo, for example (3, pp. 254-259), one cannot be certain whether the descriptions are intended to be phenotypical or genotypical; and this same thing is true of some of the examples given by J. F. Brown (1, pp. 150-154). Lewin's use of the term 'law' is also confusing. Apparently, a 'psychological law' is always a genotypic description; but in spite of the frequent use of the term no clear account is given of the method to be followed in reaching these genotypical constructs, nor are illustrations or experimental data provided.

In conclusion, I think we can safely agree with Heidebreder that the ultimate value of the topological approach to psychology must be left to experiment. More useful, perhaps, than simple pictorial analogy, topology has yet to demonstrate that one must of necessity appeal to mathematical-spatial concepts in order to arrive at a clear description of psychological events, or to obtain a better understanding of the laws governing human behavior.

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THE SOCIAL SIGNIFICANCE OF THE INTERACTION OF NEURAL LEVELS IN MAN

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INTRODUCTION

The nature of the interaction and shifting dominance of the cerebral cortex and subcortical centers is of great importance both from a purely scientific viewpoint and for considerations of social adjustment. Our primary interest in this report is scientific in character, but occasionally it is essential to digress into broader fields of application and point out fundamental implications of our research.

The functioning of the different levels of nervous control in man has a definite bearing on all our relationships with each other in social groups. A very close correspondence between cerebral action and rational behavior and between thalamic action and prejudicial, biased, emotionally inadequate behavior has been demonstrated by Cannon (1) and other investigators. If this is true, then studies on the modifiability, interaction and shifting dominance of cortical and subcortical centers has a direct bearing on the whole problem of education and the control of social functions. If a training program calls forth reactions and habits which are predominantly subcortical in character then emotional bias, unreasonable prejudice, and intolerance of the rights of others will be the dominating mode of behavior. On the other hand, if an educational program is set up which emphasizes objective, rational attitudes towards life thus calling into play predominantly the higher cortical control and producing a balance between cortical and subcortical spheres of action, then we will have a social situation wherein emotion and reason integrate for their mutual welfare.

EXTIRPATION EXPERIMENTS ON NEURAL INTERACTION

Most of the recent approaches in studying the interactions between the cerebral cortex and subcortical centers have included observations on the consequent behavior of experimental extirpation of various parts of the brain in animals. Some of these experiments have yielded some very enlightening and far-reaching results. However, experiments on intact humans on the problem of the interaction of neural levels have been conspicuously lacking because of the great difficulty in controlling the factors which are crucial in differentiating between the behavior patterns of different neural levels.

Some of the extirpation experiments on animals which are crucial to our problem will be summarized briefly. The interpretation of human behavior, however, in the light of animal behavior must be done with caution because of the great differences in allocation of functions in nervous structures of different phylogenetic levels of complexity.

Sherrington's fundamental work (16) on the differences between nerve trunk conduction and reflex arc conduction, and the interaction of different neural levels in control of the final common path is of great importance in understanding the hierarchical functioning of the nervous system as a whole. Herrick (10), Child (2), Coghill (3) and others have called attention to the analogy between the phylogenetic development of the nervous system and the stratification of neural levels in man. The subcortical, brain-stem and spinal levels in man correspond in many structural and functional aspects to the highest nerve centers in certain lower animals. The stratification of the cerebral cortex in man with its complex functions represents a later development in the phylogenetic series and according to the concept of physiological gradients the cortex would be the first neural mechanism to get out of order under conditions of great stress.

The progressive depressant effects of alcohol and certain anesthetics as greater amounts are administered, causing a descending paralysis of the central nervous system from the higher cortical centers to the lower spinal centers, have long

been recognized as operating in accordance with Jackson's law of evolution and dissolution of the nervous system. That is to say, the more recently acquired structures phylogenetically are the first to be affected by noxious stimuli, then the other nerve centers are affected in the reverse order of their phylogenetic acquisition. Rosett (15) has recently called attention to a negation of this law by pointing out the common experiences of thought, imagery and hallucination after consciousness of sensory excitation has disappeared under a general anesthetic. These purely mental experiences not only disappear later than the sensory excitations, but reappear earlier in the recovery sequence from a general anesthesia.

Lashley's earlier work (11), reported by Mettler (14, p. 397), on cerebral functions in learning in the white rat ". . . indicated that habits survived destruction of parts of cortical fields when destruction of the whole field was followed by loss." "There was no apparent qualitative change until the greater part of the area was destroyed." Driesch's term, equipotentiality, was used to describe this condition. Later, Lashley found ". . . a reduction in efficiency of performance after cerebral lesions, but without evidence for qualitative differences in performance, for lesions within a given field." This he called 'mass action,' which 'implies summation of equivalent functions.' The concept of equipotentiality, even in a specific neural field, seems to be opposed to the concept of physiological gradients in the dominance and subordination of cortical and subcortical centers. The latter concept is descriptive and apparently pretty well established in fact. Lashley's use of the term 'equipotentiality' generally has been misinterpreted. He applies the term in specific neural systems to specific functions.

These research findings indicate that the nervous system may be envisaged as having certain specific functions in certain more or less specifically circumscribed areas and in addition a unifying function which appears to cause the brain to function as a whole.

Marquis and Hilgard (12) obtained conditioned responses to light in both dogs and monkeys after total removal of the

occipital lobes. In all the dogs studied and one of the three monkeys, conditioned responses to light before the operation were present after the operation.

The decortication experiments of Culler and his associates (4, 8) on dogs have shown that the dog without a cortex is capable of crude, diffuse conditioning to gross changes in acoustic, optical, thermal and tactile stimulations but the decorticated dog is incapable of differential or adaptive conditioning.

These findings indicate that the cerebral cortex is not essential in the establishment and retention of conditioned responses in contradiction to Pavlov's earlier hypothesis. They also have great social significance in that the lower neural centers have some adaptability and docility in establishing and modifying habits.

Girden and Culler (9) demonstrated a shifting dominance of cortical and subcortical centers by testing for retention of conditioning of the ligated semitendinosus muscle while the animals (dogs) were curarized and while in the normal condition. "A conditioned response established under curare vanishes on return to the normal state, and reappears only upon re-curarization. Conversely, a CR established in the normal animal disappears under curare and reappears only upon return to normal" (p. 273). Under these conditions curarization seemed to produce a functional decortication and sub-cortical conditioning took place. The sub-cortical conditioning then disappeared when the effects of the drug disappeared due to inhibition from the cortex. Conversely, conditioning of the normal animal (with participation of the cortex) disappears under curare due to apparent temporary elimination of the function of the cortex. This experimental demonstration of the shifting dominance of cortical and sub-cortical centers is an extremely important point of departure for further studies in understanding the broader aspects of human relationships, such as dissociation, and episodes of amnesia.

STUDIES WITH INTACT HUMANS ON THE INTERACTION
OF NEURAL LEVELS

According to well established tradition in neuro-physiology, reflexes are supposed to originate at the level of the spinal cord and brain stem, instinctive behavior in the basal ganglia and thalamus, and rational or purposive behavior in the cerebral cortex. Certain experiments on intact humans have been successful in pointing out definite differences in cortically and subcortically initiated reactions. Dodge (5, 6), in his studies on the eye-lid reflex, found that an initial reflex closure of the eye-lid exhibited a latency of 35 to 40 milliseconds, followed by a partial recovery, and then another lid closure of cortical origin with a latency of 90 to 150 milliseconds. This type of temporal differentiation between cortical and subcortical activity was also found in the knee-jerk and in eye-movements.

There are many everyday examples of the dichotomy between cortical and subcortical activities. Acts which fall under the popular caption of 'absentmindedness' usually begin as subcortical, habitual responses and are then corrected by cortical interference. One starts to dress for a formal party and finds himself preparing to retire. The old veteran unconsciously drops his bundles on the street in coming to attention to a command given behind him. The novice lapses into habitual mannerisms as he tries to ape the manners and speech of a new culture. One begins to wash one's face without first removing one's spectacles. There is the tendency to lift one's felt hat by the brim after wearing a straw, or the inability successfully to disguise one's handwriting. All these habitual acts illustrate the dogged persistence of subcortical patterns and the continuous necessity of correction and interference by cortical action.

To state that a definite dichotomy exists between cortical and subcortical activities would be somewhat misleading. Lashley (11), Coghill (3), Travis and Dorsey (17) have pointed out 'mass responsiveness' of the nervous system and the overlapping functions of cortical and subcortical nerve

centers. This dichotomy perhaps can be envisaged better as a quantitative difference rather than a qualitative one. Then it becomes a matter of whether a sequence of behavioral acts is predominantly cortical or subcortical in character.

Franz and his colleagues (7), recognizing certain inadequacies in clinico-pathological and experimental-pathological approaches to cerebral localization and dominance of control, studied transfer effects in corresponding cerebral centers in intact normal human subjects. Their studies determined whether specific centers in the cortex have specific functions by intensive training of specific centers and then testing for carry-over of the same function into other centers. They found conclusive evidence of transfer of training to corresponding centers in contralateral hemispheres and spread in the homolateral centers in simple and complex visual identification of designs and words, and also in learning tactual localization.

The author (18), in studying the convergence of cortical and subcortical patterns in motor learning, found that when the cortical phase (voluntary manual pursuit) and the subcortical phase (ocular pursuit) converged into one harmonious pattern, learning was at a maximum. As long as a discrepancy existed between the cortical and subcortical reactions, the eye-hand coördination was quite inaccurate.

Figure 1 shows graphically the relationship between ocular latency, manual latency, and accuracy of coördination as the cortical and subcortical patterns converge in the learning of a task involving eye and hand adjustments. These data demonstrate that as the ocular and manual functions become progressively more harmonious, accuracy in the performance of the learning task progressively increases. As long as there is a discrepancy between the ocular latency and the manual latency, accuracy of coördination is only approximately correct.

This experimental demonstration of the necessity of convergence of cortical and subcortical action in order to produce maximum accuracy in performance is a prototype of many complex human reactions.

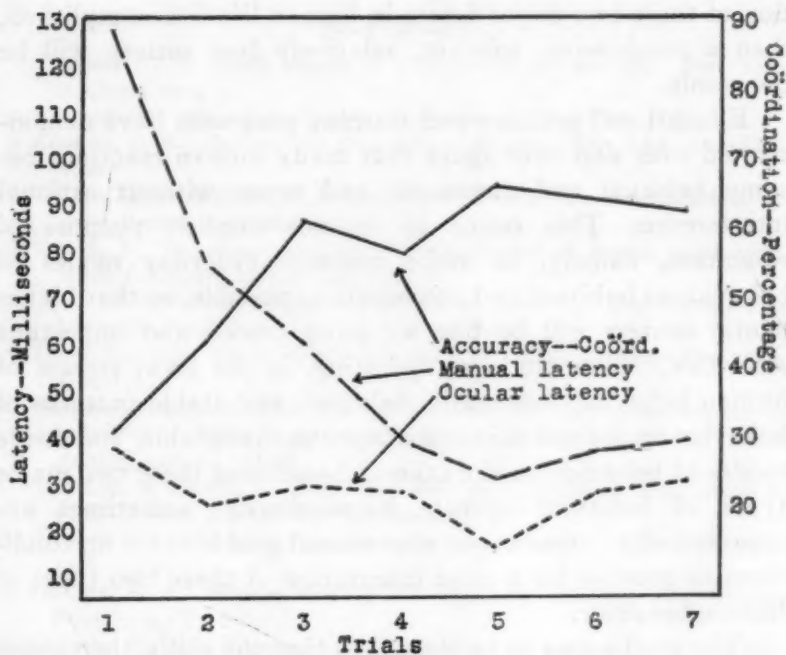


FIG. 1. Relationship between ocular latency, manual latency, and accuracy showing the convergence of cortical and subcortical functions in eye-hand coördination.

INTERPRETATIVE SUMMARY

What bearing upon our social order and what significance to education do these neurophysiological and psychological studies have?

The world in which we live is ruled not only by the latest scientific facts, but also by the predominant emotional attitudes. Perhaps it would be safer to say that elections are lost or won, wars are waged and decided, business is built up or destroyed, laws are enforced or flouted, children are nurtured or abused, homes are made happy or wretched, lives are redeemed or wrecked more by love and hate, anger and calm, pride and prejudice than by reason. In other words, the main forces which determine human events have more to do with emotion than with reason. If the prevailing emotional attitude is representative of social maladjustment then strife, turmoil and unhappiness will prevail. If the proper integra-

tion of these two major forces in human life is accomplished, then a progressive, tolerant, relatively free society will be the result.

Educational practice and training programs have demonstrated over and over again that many human reactions become habitual and automatic and occur without rational interference. This seems to be one implicit purpose of education, namely, to make common everyday modes of behavior as habitual and automatic as possible, so that higher neural centers will be free for more crucial and important activities. Thus we have operating, in the great stream of human behavior, automatic, habitual, and stable patterns of behavior on the one side and adaptive, changeable, and docile modes of behavior on the other. Sometimes these two major types of behavior operate harmoniously, sometimes antagonistically. Our major educational goal is to set up conditions to provide for a close integration of these two types of human behavior.

The road seems to be clear then that the skills, the virtues and the traits of character which we educators agree upon to teach our children must be the essential and the correct skills, virtues, and traits because once they become habitual and automatic comparatively little can be done by way of re-education. In this sense the neurophysiological character of human life is of fundamental importance and can be applied directly to the modern system of education. As was stated in the introduction, if a training program habitually calls forth reactions which are predominantly subcortical in character, then emotional bias, unreasonable prejudice, and intolerance of other people will be a dominant mode of behavior. But an educational program emphasizing objective and rational attitudes towards life and calling into play predominantly higher cortical centers of nervous control will serve the common welfare and create an integrated and well-balanced society.

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MORGAN'S CANON AND ANTHROPOMORPHISM¹

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The purpose of this paper is three-fold: It first recalls some of the historical consequences upon psychological experimentation and theory exerted by Morgan's canon; secondly, it attempts to indicate some ways in which current psychological theorizing in both the animal and human fields is violating this ancient principle; third, it shows briefly that this violation is inevitable and necessary.

We are all familiar with the fact that Darwin's theory of continuity between human and animal development gave rise to the use of the anecdotal method by Romanes in the eighteen-eighties. We are perhaps well agreed upon the necessity for some form of definite check upon the use of this method and its resulting anthropomorphism. Romanes and his followers were simply ascribing too much mind to animals and until this type of pseudo-scientific thinking was stopped really critical work in animal psychology could not get under way.

Morgan's canon was offered as just such a check. Its immediate effect was to outlaw at once any description of animal behavior as due to mental processes. It gave Loeb justification for the development of his mechanistic conception of the tropism. More than this, however, are the well-known effects of the canon on the experimental study of higher animal forms. Thorndike's experiments set the pattern for those who followed him in America. The Pavlovian conditioning technique was introduced and widely adopted as the experimental method best qualified to display the mechanisms of animal behavior. Theories of learning stemming from the conditioning technique were introduced and applied to all

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animal behavior from the opening of a problem box, to discrimination tasks, to maze learning and to delayed reaction. It thus became the fashion to deny any form of higher mental processes to all animals. As a matter of fact it became a real question for some time as to whether there was a legitimate field of animal psychology—at least in the light of what psychology at that time was supposed to be. The construction of mechanical rats and sow bugs should have taken place at about this time. In any event they are reminiscent of this particular period of animal psychology.

Only very gradually did experiments begin to appear from American laboratories in which the data forced the assumption of some form of mental processes in animals. Of these, Hunter's study of delayed reaction was among the earliest. When his study was published, however, the influence of the canon was so strong that Hunter dared give only implicit voice to the suspicion that at least some of his animals were capable of ideational processes. Slowly this type of work spread to include a variety of higher forms and a variety of more complicated problems. Finally in the case of the higher apes investigators became sufficiently bold to state and defend the proposition that these forms were undoubtedly using what to all intents and purposes were ideational processes.

After having reached this point some investigators turned their attention to a more intensive study of the rat. Within the last decade a considerable number of studies have appeared in which even the rat's behavior has been described in terms of mental processes. I might mention in this connection the work of Tolman, Krechevsky, and Maier. What would have happened to these hardy souls had they published during the early years of our twentieth century? Perhaps the way in which Krechevsky puts his 'hypotheses' in quotes indicates what he thinks might happen to him even yet if he omitted them. Tolman also tries to develop a verbal defense against his being accused of anthropomorphism. He asserts that the terms he employs are needed to give an intelligible description of his animal's behavior. They are open to check by any one who is interested. They are operationally defined. In spite

of all this, however, the fact remains that the terms he uses are much the same type of terms used to describe human behavior. In other words, and bluntly, Tolman is anthropomorphic.

Maier might not like to be accused of being anthropomorphic but the fact remains that the recent prize earned by him was earned because he anthropomorphized his animals' behavior. The term neurosis, or neurotic, is meaningful only in terms of the concepts and phenomena of the field of abnormal psychology. True, he might try to justify the term by some such verbal appeal as does Tolman but a neurosis is not, for all that, something to be ascribed to mechanical rats.

It is not necessary to spend more time in calling more witnesses of current anthropomorphic descriptions of animal behavior before us. They would only further support the conclusion that anthropomorphism is with us today. The fact that their work is being published gives ample proof of the fact that anthropomorphism is at present not the crime it was once considered to be.

If this is a true statement of present-day animal theorizing what are we to make of it? Have we been travelling in a great circle? Have we returned to what has been styled a medieval and lifeless tradition? Have we rashly thrown out mind, consciousness, and mental process only to welcome them back? Hardly. The anthropomorphism in animal psychology today is not the anthropomorphism intended in Morgan's canon. That anthropomorphism came about as a result of the failure to observe adequate scientific control of the behavior studied; as an inevitable result of the use of the anecdotal method; as a result of the failure to take into consideration the habits and past training of the animal whose behavior was being reported. Surely we cannot accuse our contemporaries of using the anecdotal method. Certainly they are well acquainted with the habits and training of their animals, and I have yet to hear serious criticism of the experimental methods employed. The simple truth seems to be that we must anthropomorphize to give an adequate picture

of some forms of animal behavior. In a word, some anthropomorphism is forced upon us by the animals themselves.

My own attitude toward current anthropomorphism in animal work may be summarized as follows: Valuable work is being done by those in the animal field who have thrown off an original servile obedience to Morgan's canon. It seems to me that the purpose for which it was originally formulated has been fulfilled. Its subsequent history has taught us that if unbridled anthropomorphism is bad so likewise is no anthropomorphism in the animal field. Unless we do employ it the experimental study of some important problems in the animal field is blocked.

Let us now turn to a consideration of another phase of the influence of Morgan's canon. This started at about the time that the debate over the existence of animal psychology was raging. Impressed by the seeming scientific progress made in the animal field with the objective and mechanistic methods Watsonian Behaviorism had its hey-day. The behaviorists were not satisfied with the rejection of anthropomorphism in the field of animal behavior. They had to carry this denial over to the human field as well. This resulted in a paradoxical situation. No longer could human behavior be interpreted in terms of mental process or, in other words, human processes could not be utilized in explaining human behavior! Thus did Morgan's canon backfire. It became a boomerang and for a number of years textbooks were written in which every effort was made to avoid the use of any terms which might be interpreted to refer to mental processes. If any such terms were employed the author would say that he had 'carefully defined them in terms of the behavior of the total organism.' It was during this time that psychology lost its mind, its consciousness, its memory, and its reason. Proponents of behaviorism and its offspring, objectivism, saw in these terms a distressing allegiance to vitalistic and theological concepts.

The result of this attempt to escape anthropomorphism was the introduction of such definitions as the following in serious psychological papers:² "Mind is a term of various

² These are direct quotations. No references are cited because to do so would tend to introduce personalities which I have tried to avoid.

significations, not employed in accurate psychological discourse"; "Mental activity is protoplasmic activity"; "Organization in a sensory field is something which originates as a characteristic achievement of the nervous system"; "Learning is a changing of connections at synapses . . . it is a neural phenomenon"; "From the scratch reflex of a dog's hind leg it may seem a far cry to such a phenomenon as a man's choosing caviar from his menu card, or marking the Republican ticket, or falling in love, or thinking out the relative merits of monism and epiphenomenalism—but the difference is only a difference of degree"; "Frustration is said to occur when—an excitatory tendency in a behavior sequence encounters a situation which makes the execution of the act impossible"; "An attempt is a segment of behavior the termination of which is marked by either reinforcement or extinction"; "Disappointment is the diminution in the power of one reinforcing situation to evoke appropriate consummatory reaction"; and this last "The brain is tuned to see motion, and grabs at any chance to see it. It sees motion much more readily than it can pick out the successive positions through which a moving body passes."

I am here simply recording what anyone can find in current and recent textbooks. I do not propose to be put in the position of either defending or criticizing the above statements at present. These statements are illustrative of any number that might be selected. Without exception they represent an anthropomorphism of the specific structures named by each, an anthropomorphism committed in an attempt to avoid anthropomorphism. In other words theorists apparently are unable to escape it. In most of the instances cited they have denied psychological processes to the human individual but have ended by ascribing them to the nervous system. In this sense it may be said that we psychologists have ended by out-anthropomorphizing the anthropomorphists against whom Morgan issued his canon.

Once again we may ask, if this is a true statement of affairs in human psychology what is to be done about it? Let me reiterate that I am not condemning or applauding the ten-

dency. I consider it inevitable that anthropomorphism must be used. We can make intelligible some forms of animal behavior only by describing it in anthropomorphic terms. Likewise an adequate description of the living, moving, fluid, ever-changing behavior of the human individual can be given only in terms of anthropomorphic or mental terms. These are the only terms we know, since we can interpret phenomena only in terms of our own experience. We can interpret learning only in terms of our own learning, we can interpret the brain grabbing at the chance to see motion only in terms of our own grabbing after a dollar bill, and so on. Thus we cannot escape anthropomorphism. Its use is therefore inevitable and necessary.

But let us now return to the examples of anthropomorphizing cited above. My last comments lead to the conclusion that we are forced to be anthropomorphic in our descriptions of behavior. But it can now be a clearer-headed type of anthropomorphism. We need no longer apologize for it, defend or conceal it. Psychoanalytically we might suspect that for a number of years we have been compensating for this unconscious need for anthropomorphism to the point of neuroticism. This neurosis may now confidently be expected to clear up and psychologists and psychology may become normal again. It is my own personal bias to believe that the first step toward returning normality will be taken when we cease personifying the nervous system. This system does not, never has, and never will act in isolation. Why burden it with a load that only the organism-as-a-whole can with difficulty carry? In a way we may describe the animal psychologist as already having reached this point in his thinking. In animal psychology investigators are primarily interested in what the animal does. Their study of the nervous system is undertaken not as a key to the solution and complete explanation of behavior but rather as a means of throwing the behavior studied into relief. I need not stop at this point to call the roll of those who are studying the function of cerebral injury to reasoning, to the formation of hypotheses, to proactive inhibition, to memory, learning, and the rest. The

point is that their operational techniques are done to clarify certain features of the animal's behavior—not to explain it. The nervous system is considered as representing only one of the conditions producing the behavior. In human psychology we need to adopt this same attitude more extensively. We should be interested in the behavior of the organism-as-a-whole. This should be our goal. Only the human individual can legitimately be the object of our unavoidable anthropomorphism. Our studies of the nervous system are legitimate, certainly, but their psychological importance lies in the degree to which they make the conditions of human behavior clearer.

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ON CONSTANCY OF VISUAL SPEED¹

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When investigating the constancy of visual speed J. F. Brown² discovered what he called the transposition principle of velocity. In his account the constancy of visual speed and the principle of transposition occur as unrelated facts. This paper attempts to show that constancy of visual speed can be understood as a consequence of the transposition principle.

When objects which move with the same objective velocity are presented to the resting eye at different distances one perceives them as moving with approximately equal speed, although the displacements of their retinal images per unit of time vary in inverse proportion to the distance. This is what we call the constancy of visual speed. Its formal similarity to the constancy of size is obvious. Two identical objects presented at different but moderate distances from the eye have almost equal phenomenal sizes, although the linear extensions of the corresponding images are inversely proportional to the distances at which the two objects are presented to the eye. It seems plausible to assume that constancy of visual speed is simply a consequence of size constancy. One might argue that visual speed depends not on the length through which an image passes on the retina per unit of time but on the visual extension through which the object moves. Since the latter extension remains approximately constant even if its objective size is projected from different distances and therefore with varying retinal size, the constancy of visual speed seems to follow without any further assumptions.

¹ The writer wishes to express his appreciation to Professor Wolfgang Köhler and also to Dr. Richard S. Crutchfield for their aid in preparation of this paper.

² J. F. Brown. Ueber gesehene Geschwindigkeiten. *Psychol. Forsch.*, 1927, 10, 84-101. Also J. F. Brown, The visual perception of velocity. *Psychol. Forsch.*, 1931, 14, 199-232.

This was indeed the reasoning which led J. F. Brown to his investigation of the constancy of speed. His actual observations, however, did not entirely confirm this view. While with moderate distances and under otherwise favorable conditions size constancy is almost absolute, constancy of speed proved to be considerably less perfect. When two objects moved at different distances from the eye, the objective velocity of the more distant object had to be distinctly greater, if the two phenomenal speeds were to appear as equal. Brown concluded that the constancy of speed cannot simply be deduced from the constancy of size. He therefore began a thorough investigation of "the factors that condition phenomenal velocity."

In his experiments Brown had his observers compare the speeds in two movement-fields which from experiment to experiment differed in various respects. Probably his most important finding is the transposition principle, which he established in experiments in which the two movement-fields differed only with respect to their size, being transposed in all their linear dimensions in a certain proportion. A movement-field consisted of an opening in a black cardboard screen and black dots of equal size moving through this opening on a white background. The dots were pasted on a roll of white paper running over two moving drums which were hidden by the screen. The drums were far enough apart so that only a flat surface was visible through the opening. The field in the opening was uniformly illuminated. The surface of the paper was smooth so that no cues of its motion could be obtained, except from the dots. The observer was to compare successively the speed with which the dots in two such movement-fields passed through their respective openings. In one of the fields the velocity was variable and could be stepped upwards or downwards under the direction of the observer until the speed in the two movement-fields appeared to be the same. Then the velocities were measured and their quotient was computed. The movement-fields were placed far enough apart so that only one could be seen at a time.

In one of these experiments, for instance, the movement-

fields were transposed in a ratio 2 : 1, *i.e.* all the linear measures in one of the moving fields, namely the size of the opening, the diameter of the dots and their distance from one another, were twice as large as the same measures in the other moving field. After the objective velocities were so adjusted that the phenomenal speed in the two movement-fields was the same, the objective velocity in the larger field (*A*) was found to be almost twice as great as in the smaller field (*B*). Where V_A is the velocity in *A* and V_B the velocity in *B* when phenomenal equality is attained, $\frac{V_A}{V_B}$ was found to be 1.94

(average for 7 observers).³ When the spatial transposition of the two movement-fields was 4 : 1, the speeds in the two fields were judged to be equal when the ratio of the objective velocities was 3.7 (average for 5 observers).⁴ Thus the objective velocity in the 4 times larger field *A* was approximately 4 times as great as was that in the smaller field *B*, when visually both movements seemed to have the same speed. On the basis of these results Brown formulated the principle of velocity transposition: If a movement-field in a homogeneous surrounding field is transposed in its linear dimensions in a certain proportion, the stimulus velocity must be transposed in the same proportion in order that the phenomenal speed in both cases be identical.

The velocity ratios actually measured by Brown departed significantly from the figures called for by this principle, particularly when the difference in the dimensions of the two movement fields was still larger. When the transposition was in the proportion 10 : 1, the ratio of the velocities was 8.22⁵ (average for 4 observers; *cf.* below for additional results). Still these various departures from the theoretically expected values seem very small when we compare them with the enormous effects of the transposition phenomenon which were

³ J. F. Brown. Ueber gesehene Geschwindigkeiten. *Psychol. Forsch.*, 1927, 10, p. 91, Table 5.

⁴ *Ibid.*, p. 92, Table 8; also J. F. Brown, The visual perception of velocity. *Psychol. Forsch.*, 1931, 14, p. 216.

⁵ J. F. Brown. The visual perception of velocity. *Psychol. Forsch.*, 1931, 14, p. 216.

actually found. In the last mentioned case with a transposition of 10 : 1 where the departure from the expected value was 18 per cent, the actually measured effect of the transposition phenomenon was as high as 722 per cent.

In a quite similar way Brown had previously determined to what degree constancy of speed is actually realized. Two identical movement-fields were placed at different distances from the observer and the ratio of their velocities was varied until the speeds in the two fields seemed to be equal. The movement in the more distant field was then 1.12, 1.15, and 1.21 times faster than the other, where the ratio of the distances from the observer was 1 : 3.3, 1 : 6.6 and 1 : 10 respectively.⁶ Perfect constancy, of course, would have yielded in each case the ratio 1 instead of the listed quotients. Again, the actually found figures depart only little from the values to be expected for perfect constancy, when we compare them with the values which we should find if phenomenal speed were proportional to the velocities on the retina. On the other hand the departure from ideal constancy is here significantly larger than the departure which size constancy shows, a difference great enough to justify Brown's conclusion that constancy of speed cannot be deduced from size constancy.

We are thus facing an apparently paradoxical state of affairs. On the one hand we find a speed constancy of high degree, when speeds in movement-fields at different distances from the eye are compared; on the other hand the transposition experiments show that at a constant distance objective velocities may appear equal when one is as much as 8 times faster than the other. The fact that the reported transposition experiments were done under unnatural dark-room conditions affords no comfort. When Brown repeated the experiments with daylight illumination so that the continuity of the spatial framework was plainly given, he obtained for the same ratios of transposition, namely 2 : 1, 4 : 1 and 10 : 1, the velocity ratios 1.57, 2.71 and 6.17.⁷ Even under these condi-

⁶ *Ibid.*, p. 208, Table 1.

⁷ *Ibid.*, p. 215, Table 7.

tions of adequately structured visual field the transposition phenomenon remains striking.

In an intricate state of affairs like this the first thing to do is to examine closely the immediate stimulus situation. It is in the present case represented by the retinal images of the movement-fields. In a transposition experiment the retinal images of the two movement-fields bear to each other the same proportion as the objective movement-fields themselves, and the rates of the shifting dots on the retina are also proportional to the objective velocities. In the constancy experiment the situation is different in that here the movement-fields are presented at different distances from the eye, and the retinal images have different sizes, although they correspond to objectively identical fields. More specifically, their dimensions are inversely proportional to the distances at which the corresponding movement-fields are presented. When, for instance, of two identical movement-fields, *A* is presented at 2 m. distance and *B* at 4 m. distance, the image of *A* is linearly twice as large as the image of *B*. Let us assume for the moment that constancy of speed is perfect, so that the speed in the fields *A* and *B* would seem to be the same when the objective velocities are equal. Since displacements in *A* and *B* produce retinal displacements which are twice as large in the case of *A* as they are in the case of *B*, phenomenal speeds are equal when the *retinal velocity in A is twice as great as that in B*. Let us now consider a case of the transposition phenomenon under the assumption that the principle of transposition also holds perfectly. If *A'* be a movement-field twice as large in all dimensions as *B'* and if both be presented at the same distance from the eye, the retinal image of *A'* is twice as large as that of *B'*. According to the transposition principle, the phenomenal speed in both fields is the same when the objective velocity in *A'* is twice as great as in *B'*. This being the case, *the velocity in the retinal image of field A' is also twice as great as is that in the retinal image of B'*. We thus find that the two different experimental situations yield essentially the same processes *on the retina*. The constellations of phenomenal equality in the constancy experiment on the

one hand and in a transposition experiment on the other hand, both referred to the retina, are exactly alike. Thus, if we apply the principle of transposition *to the retinal images* of the two movement-fields in a constancy experiment, this principle leads to equality of phenomenal speed, *i.e.*, to just the fact which is commonly called constancy of speed. In this manner constancy of speed can be explained without any further hypothesis. Incidentally, in this explanation there is no reference to constancy of size. *The transposition principle alone, if applied to retinal images and retinal displacements, yields constancy of visual speed.*

In this connection, it may be useful to give the transposition principle another formulation. Velocity is usually measured as displacement per unit of time. We then may say: In movement-fields of identical shape and different dimensions the phenomenal speed is the same when the displacements per unit of time are equal fractions of the respective openings. Or simply: In transposed movement-fields, the perceived speeds are the same when the *relative* displacements are equal. Since in a transposition experiment the retinal images of the movement-fields have the same size proportions as the actually presented movement-fields, the principle applies directly to the retinal images. On the other hand, if in a constancy experiment the distance of a field *A* from the eye is half that of an identical field *B*, the retinal image of *A* is linearly twice as large as that of *B*. According to our principle, the two images will again yield the same phenomenal speed when the retinal displacements per unit of time cover equal fractions of their respective movement-fields on the retina. What does this mean in objective physical terms? The very problem of constancy of speed arises from the fact that the same physical displacement causes different retinal displacements, depending upon the objective distance of the movement-field. More concretely, the retinal displacements are inversely proportional to the distance of the field. But, as I just mentioned, the retinal image of the field itself is also linearly in inverse proportion to the distance. Consequently the retinal displacement per unit of time remains a constant fraction of the

retinal movement-field when in objectively identical fields the same objective velocity is given at varying distances. Thus, from the point of view of the transposition principle, the condition for constant phenomenal speed is fulfilled precisely when objective circumstances are those of constancy of speed.

Actually, constancy of speed is not perfect. But the results of transposition experiments, too, fall somewhat short of exact proportionality as shown by the figures that have been quoted. In the case of the transposition phenomenon, Brown attributes the departures from the ideal values to defective homogeneity of the surrounding fields. Although as a rule the transposition experiments are performed under darkroom conditions, the illumination of the movement-fields themselves somewhat lightens the surroundings. That inhomogeneity of the surrounding fields reduces the transposition phenomenon is one of Brown's well-established results. He reports 3 series of transposition experiments under different conditions of illumination. We shall quote here only the results which he obtained with a transposition ratio of 10 : 1. They are representative for the trend in the 3 series. One experiment was made in daylight illumination, and gave the velocity ratio $\frac{V_A}{V_n}$ 6.17, where V_A refers to the 10 times larger field. Another experiment was done in a dark room, but the illumination of the movement-fields somewhat lightened the surroundings of the fields. This had a definite effect on the result, as Brown points out conclusively.⁸ $\frac{V_A}{V_n}$ was here 6.83.

In the third series the illumination of the movement-fields "was cut down considerably so that the surrounding fields approached homogeneity." The ratio here obtained was as high as 8.22. Indeed the departure from the ideal ratio (which would here be 10) is doubled when the observation is made with daylight illumination (6.17 as against 8.22). Brown was able to obtain a further decrease in proportionality. He covered the two cardboards in which the openings of the

⁸ *Ibid.*, p. 216, discussion of curve *b*.

movement-fields were cut with a wallpaper which showed a regular geometric pattern. He then repeated the experiment, using an objective transposition ratio of 4 : 1, and of course daylight illumination. The resulting velocity ratio now was approximately 2, whereas the same pair of movement-fields gave a ratio of 2.7 when, again in daylight, a homogeneous black cardboard surrounded the movement-fields.⁹

This experiment clearly demonstrates the manner in which an inhomogeneous environment influences the velocity ratio. Such an environment disturbs the simple proportionality of the movement-fields. Phenomenal speeds are equal when the displacements per unit of time are the same in proportion to the dimensions of their respective fields. If both fields are surrounded by the same pattern, a common framework is introduced which will tend to equalize conditions and thus to reduce the influence of transposition. In the case of the ordinary daylight experiment the outer edge of the two equal cardboards is introduced as such a common framework.

The departure from perfect constancy of speed is not much discussed in Brown's paper. Constancies are rarely quite complete. Some authors attribute almost explanatory significance to the fact that actually visual size, brightness and shape lie somewhere between the properties of the 'real' objects and properties corresponding to the retinal situation. According to our discussion, constancy of speed is no longer an independent fact but rather a by-product of the transposition phenomenon. It is in this light that we have to discuss the departure from perfect constancy of speed.

Generally, constancy of size and of shape are enhanced when one changes from darkroom conditions to daylight illumination. For the transposition phenomenon the opposite is true. It decreases upon such a change. It should be interesting to note in what way constancy of speed reacts to changes of illumination. The figures quoted by Brown for speed constancy under the two different conditions show *no* significant difference.¹⁰ At the first glance, it may seem

⁹ *Ibid.*, p. 218.

¹⁰ *Ibid.*, p. 208 f., Tables 1 and 2.

surprising that daylight illumination has not the same unfavorable effect on speed constancy as it has on the transposition phenomenon, when the two facts are interpreted as being fundamentally the same thing. But when we consider the matter again in terms of retinal images, and recall the way in which daylight conditions influence the transposition phenomenon, we find this result of Brown in line with our notions. Daylight conditions disturb the transposition phenomenon by introducing unproportional (equal) elements in the environment of the movement-fields. There should be no such unfavorable effect when strictly *transposed* surroundings are added to the transposed movement-fields proper. And this is what daylight illumination actually does in a constancy experiment. Here the movement-fields are objectively identical, and the transposed sizes of the retinal images are due to the fact that they are projected from different distances. But the same holds for the objectively identical forms in the immediate surroundings of the movement-fields, as, for instance, the edges of the cardboard screens and the supporting tables. Their retinal images are transposed in the same ratio as are the movement-fields themselves. In this way only proportional elements are added to the transposed movement-fields, and these cannot impair the effect of the transposition principle. On the other hand, they do not seem to improve it either. Brown's results, according to which the departure from ideal constancy is about the same for daylight illumination as for darkroom conditions, indicate that the addition of proportional elements does not serve to increase the effect of the transposition principle. Obviously, the movement-fields as such furnish a framework which guarantees this effect, and not much is changed when further proportional structures are added on the retina.

On the other hand it remains true that neither constancy of speed nor the transposition principle is completely realized. We have seen that in the case of speed constancy the deviations are not due to additional structures in the environment. We may therefore doubt whether in the case

of transposition unproportional elements are *entirely* responsible for the deviations.

Incidentally, when we compare the departure from perfect constancy with the departure from complete transposition in the results of Brown's transposition experiments, we find that one of Brown's darkroom series yields about the same departure from the ideal transposition values as was found in corresponding constancy experiments.

For the purpose of such a comparison we consider again the velocities on the retina. Unfortunately, there is only one case in which transposition in the dark room and an experiment on constancy can be strictly compared; a constancy experiment in which the ratio of the distances of the movement-fields from the observer was 1 : 10, and a transposition experiment in which the movement-fields were transposed in the ratio 10 : 1. In both cases the retinal images of the movement-fields bear the same size proportions. In the constancy experiment equality of speed was attained when the velocity in the more distant field was 1.21 of that in the nearer (average of five observers, Table 1).¹¹ This means that the retinal velocity corresponding to the more distant field was .121 of that corresponding to the other; for, the image of the more distant field was one-tenth of the size of the other field, and the same, of course, was true of the retinal displacements. With this figure we have to compare the result for the size ratio 10 : 1 when transposition was measured in a dark and nearly homogeneous room. The velocity ratio here obtained was 8.22 (average of four observers). The ratio of the retinal velocities which we computed for the corresponding constancy experiment was .121. This is the quotient of the velocity in the smaller retinal movement field and the velocity in the larger one. Since Brown presents the velocity ratios for transposition experiments in the converse fashion (velocity in the larger field divided by that in the smaller field), we have to express the result of the constancy experiment as $1/.121$ instead of .121. If this is done the figures become comparable. The value of $1/.121$ is 8.26, in notable agreement with 8.22, the result of the transposition experiment.

In a constancy experiment with the distance ratio 1 : 5 the ratio of the objective velocities was 1.14 when phenomenal equality was attained (average of four observers, Table 2).¹² For the corre-

¹¹ *Ibid.*, p. 208.

¹² *Ibid.*, p. 209.

sponding transposition ratio, 5 : 1 no data are available from the darkroom series in question. But the velocity ratio 3.7 for the transposition ratio 4 : 1, which is rather close to 5 : 1, taken together with the ratio 8.22 for 10 : 1,¹³ permits by interpolation the computation of the value for the ratio 5 : 1, which is 4.45. In order to make the result of the constancy experiment, namely 1.14, comparable to this figure, we reduce this to retinal velocities and take again the reciprocal value (*cf.* above). The result is 4.39, again a close agreement.

We may conclude from these cases of agreement, that the transposition experiments to which they refer were done under optimal conditions; *i.e.*, that a further decrease in the illumination would not improve the transposition of velocities in movement-fields of different sizes. For, the results of these transposition experiments correspond exactly to those of the constancy experiments with which they were compared. And in these, we have seen, results were optimal because all additional structures were properly transposed on the retina by virtue of the essential experimental conditions.

We now have to ask ourselves what factors limit the exact validity of the transposition principle. Recently D. Cartwright¹⁴ was able to show that the difference threshold for the position of a point within an opening exhibits the same dependence on the properties of the opening as does phenomenal speed. In a three times larger opening the threshold for changes of position of a point was found to be 2.7 times larger than that in a smaller opening. Approximately the same ratio was obtained by Brown, when he determined the physical velocities which gave equal phenomenal speeds in openings of the relative sizes 3 and 1. In a second instance a similar agreement was found between the ratio of velocities which yielded equal phenomenal speeds, and the ratio of the thresholds of position measured under comparable conditions. This parallelism suggests a close relationship between visual speed and the threshold for changes of position. Actually, several phenomena in the field of visual speed can be explained, if we realize that our sensitivity for changes of position de-

¹³ *Cf.* above.

¹⁴ D. Cartwright. On visual speed. *Psychol. Forsch.*, 1938, 22, 320-342.

depends on a great many factors. Here it seems relevant that this sensitivity follows Weber's law within certain limits. Strict validity of Weber's law for spatial changes would mean that the thresholds for changes of position is proportional to the size of the openings in which the threshold is measured. From this point of view one might expect the transposition principle of velocities to be fully realized. Actually, Weber's law does not strictly hold in this field. This follows clearly from Cartwright's experiments. But the departure from Weber's law seems to be of about the same magnitude as the departure from ideal transposition of velocity. Thus the departure from ideal transposition of velocity may be attributed to the fact that Weber's law does not strictly hold in the case of spatial changes.

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A STIMULUS-RESPONSE ANALYSIS OF ANXIETY AND ITS ROLE AS A REINFORCING AGENT¹

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Within recent decades an important change has taken place in the scientific view of anxiety (fear),² its genesis, and its psychological significance. Writing in 1890, William James (6) stoutly supported the then current supposition that anxiety was an *instinctive* ('idiopathic') reaction to certain objects or situations, which might or might not represent real danger. To the extent that the instinctively given, predetermined objects of anxiety were indeed dangerous, anxiety reactions had biological utility and could be accounted for as an evolutionary product of the struggle for existence. On the other hand, there were, James assumed, also anxiety reactions that were altogether senseless and which, conjecturally, came about through Nature's imperfect wisdom. But in all cases, an anxiety reaction was regarded as phylogenetically fixed and unlearned. The fact that children may show no fear of a given type of object, *e.g.*, live frogs, during the first year of life but may later manifest such a reaction, James attributed to the 'ripening' of the fear-of-live-frogs instinct; and the fact that such fears, once they have 'ripened,' may also disappear he explained on the assumption that all instincts, after putting in an appearance and, as it were, placing themselves at the individual's disposal, tend to undergo a kind of obliviscence or decay unless taken advantage of and made 'habitual.'

¹This paper, in substantially its present form, was presented before the Monday Night Group of the Institute of Human Relations, Yale University, March 13, 1939.

²Psychoanalytic writers sometimes differentiate between anxiety and fear on the grounds that fear has a consciously perceived object and anxiety does not. Although this distinction may be useful for some purposes, these two terms will be used in the present paper as strictly synonymous.

Some years later John B. Watson (11) demonstrated experimentally that, contrary to the Jamesian view, most human fears are specifically relatable to and dependent upon individual experience. Starting with the reaction of infants to loud sounds or loss of physical support, which he refused to call 'instinctive' but did not hesitate to regard as 'unlearned' or 'reflexive,' Watson was able to show, by means of Pavlov's conditioning technique, that an indefinitely wide range of other stimuli, if associated with this reaction, could be made to acquire the capacity to elicit unmistakably fearful behavior. This was an important discovery, but it appears to have involved a basic fallacy. Watson overlooked the fact that 'loud sounds' are intrinsically *painful*, and he also overlooked the fact that 'loss of physical support,' although not painful in its own right, is almost certain to be followed by some form of stimulation (incident to the stopping of the body's fall) that is painful. The so-called fearful reaction to loss of support—if not confused with an actual pain reaction—is, therefore, in all probability itself a learned (conditioned) reaction, which means that, according to Watson's observations, human infants show no innate *fear* responses whatever, merely innate *pain* responses.

Freud seems to have seen the problem in this light from the outset and accordingly posited that *all* anxiety (fear) reactions are probably learned;³ his hypothesis, when recast in stimulus-response terminology, runs as follows. A so-called 'traumatic' ('painful') stimulus (arising either from external injury, of whatever kind, or from severe organic need) impinges upon the organism and produces a more or less violent defence (striving) reaction. Furthermore, such a stimulus-response sequence is usually preceded or accompanied by originally 'indifferent' stimuli which, however, after one or more temporally contiguous associations with the traumatic stimulus, begin to be perceived as 'danger signals.'

³ Freud (3) has explicitly acknowledged the possibility of anxiety occurring, especially in birds and other wild animals, as an instinctive reaction; but he takes the position that in human beings, instinctive anxiety (not to be confused with 'instinctual' anxiety, *i.e.*, fear of the intensity of one's own organic impulses) is probably non-existent or is at least inconsequential.

i.e., acquire the capacity to elicit an 'anxiety' reaction. This latter reaction, which may or may not be grossly observable, has two outstanding characteristics: (i) it creates or, perhaps more accurately, consists of a state of heightened tension (or 'attention') and a more or less specific readiness for (expectation of) the impending traumatic stimulus; and (ii), by virtue of the fact that such a state of tension is itself a form of discomfort, it adaptively motivates the organism to escape from the danger situation, thereby lessening the intensity of the tension (anxiety) and also probably decreasing the chances of encountering the traumatic stimulus. In short, *anxiety (fear) is the conditioned form of the pain reaction*, which has the highly useful function of motivating and reinforcing behavior that tends to avoid or prevent the recurrence of the pain-producing (unconditioned) stimulus.

In the mentalistic terminology that he characteristically employs, Freud (3) has formulated this view of anxiety formation and its adaptational significance as follows:

"Now it is an important advance in self-protection when this traumatic situation of helplessness [discomfort] is not merely awaited but is foreseen, anticipated. Let us call the situation in which resides the cause of this anticipation the danger situation; it is in this latter that the signal of anxiety is given. What this means is: I anticipate that a situation of helplessness [discomfort] will come about, or the present situation reminds me of one of the traumatic experiences which I have previously undergone. Hence I will anticipate this trauma; I will act as if it were already present as long as there is still time to avert it. Anxiety, therefore, is the expectation of the trauma on the one hand, and on the other, an attenuated repetition of it" (pp. 149-150).

"Affective [anxiety] states are incorporated into the life of the psyche as precipitates of primal traumatic experiences, and are evoked in similar situations like memory symbols" (p. 23). "Anxiety is undeniably related to expectation; one feels anxiety *lest* something occur" (pp. 146-147).

According to views expressed elsewhere by Freud, expectation and anxiety lie along a continuum, with the former merging into the latter at the point at which it becomes uncomfortably intense, *i.e.*, begins to take on motivational properties in its own right. The preparatory, expectant character of anxiety is likely, however, to be obscured by the fact that danger situations sometimes arise and pass so quickly that they are over before the anxiety reaction—involving, as it does, not only an augmentation of neuro-muscular readiness and tension but also a general mobilization of the physical energies needed to sustain strenuous action—has had an opportunity to occur. The result is that in situations in which danger is so highly transitory, as, for example, in near-accidents in motor traffic, anxiety is commonly experienced, somewhat paradoxically, *after* the danger is past and therefore gives the appearance of being indeed a useless, wasted reaction (*cf.* James). It must not be overlooked, however, that situations of this kind are more or less anomalous. The fact that in a given situation the element of danger disappears before flight, for which the anxiety-preparedness is most appropriate, has had time to occur, does not, of course, mean that anxiety-preparedness in the face of danger is not in general a very adaptive reaction.⁴

As early as 1903, Pavlov (10) expressed a point of view that bears a striking resemblance to the position taken by Freud in this connection. He said: "The importance of the remote signs (signals) of objects can be easily recognized in the movement reaction of the animal. By means of distant and even accidental characteristics of objects the animal seeks his food, avoids enemies, etc." (p. 52). Again, a quarter of a century later, Pavlov (9) wrote as follows:

"It is pretty evident that under natural conditions the normal animal must respond not only to stimuli which themselves bring immediate benefit or harm, but also to other physical or chemical agencies—waves of sound, light, and the like—which in themselves only *signal* the approach of these stimuli; though it is not the sight and

⁴ *Cf.* the discussion of the 'startle pattern' by Landis and Hunt (5).

sound of the beast of prey which is in itself harmful to the smaller animal, but its teeth and claws" (p. 14).

Although both Pavlov and Freud thus clearly recognize the biological utility of anticipatory reactions to danger signals, there is, however, an important difference in their viewpoints. Pavlov emphasizes the mechanism of simple stimulus substitution (conditioning). According to his hypothesis, a danger signal (the conditioned stimulus) comes to elicit essentially the *same* 'movement reaction' that has previously been produced by actual trauma (the unconditioned stimulus). It is true that the blink of the eyelids to a threatening visual stimulus is not greatly unlike the reaction made to direct corneal irritation. A dog may learn to flex its leg in response to a formerly neutral stimulus so as to simulate the flexion produced by an electric shock administered to its paw. And a small child may for a time make very much the same type of withdrawal reactions to the sight of a flame that it makes to actual contact with it. However, any attempt to establish this pattern of stimulus substitution as the prototype of all learning places severe restrictions on the limits of adaptive behavior: it implies that the only reactions that can become attached to formerly unrelated stimuli (*i.e.*, can be learned) are those which already occur more or less reflexly to some other type of stimulation.

According to the conception of anxiety proposed by Freud, on the other hand, a danger signal may come to produce any of an infinite variety of reactions that are wholly unlike the reaction that occurs to the actual trauma of which the signal is premonitory. Freud assumes that the first and most immediate response to a danger signal is not a complete, overt reaction, as Pavlov implies, but an implicit state of tension and augmented preparedness for action,⁵ which he calls 'anxiety.' This state of affairs, being itself a source of discomfort, may then motivate innumerable random acts, from which will be selected and fixated (by the law of effect) the behavior that most effectively reduces the anxiety. Anxiety is thus to be

⁵ Cf. the revised theory of conditioning proposed by Culler (2).

regarded as a motivating and reinforcing (fixating) agent, similar to hunger, thirst, sex, temperature deviations, and the many other forms of discomfort that harass living organisms, which is, however, presumably distinctive in that it is derived from (based upon anticipation of) these other, more basic forms of discomfort.⁶

By and large, behavior that reduces anxiety also operates to lessen the danger that it presages. An antelope that scents a panther is likely not only to feel less uneasy (anxious) if it moves out of the range of the odor of the panther but is also likely to be in fact somewhat safer. A primitive village that is threatened by marauding men or beasts sleeps better after it has surrounded itself with a deep moat or a sturdy stockade. And a modern mother is made emotionally more comfortable after her child has been properly vaccinated against a dreaded disease. This capacity to be made uncomfortable by the mere prospect of traumatic experiences, in advance of their actual occurrence (or recurrence), and to be motivated thereby to take realistic precautions against them, is unquestionably a tremendously important and useful psychological mechanism, and the fact that the forward-looking, anxiety-arousing propensity of the human mind is more highly developed than it is in lower animals probably accounts for many of man's unique accomplishments. But it also accounts for some of his most conspicuous failures.

The ostrich has become a proverbial object of contempt and a symbol of stupidity because of its alleged tendency, when frightened, to put its head in the sand, thereby calming its emotional agitation but not in the slightest degree altering the danger situation in its objective aspects. Such relevant scientific inquiry as has been carried out indicates, however, that infra-human organisms are ordinarily more realistic in this respect than are human beings. For example, if a dog learns to avoid an electric shock by lifting its foreleg in response to a tone, it will give up this response entirely when it discovers that the tone is no longer followed by shock if the

⁶ Freud has never explicitly formulated this view in precisely these words, but it is clearly implied in various of his writings.

response is not made. Human beings, on the other hand, are notoriously prone to engage in all manner of magical, superstitious, and propitiatory acts, which undoubtedly relieve dread and uncertainty (at least temporarily) but which have a highly questionable value in controlling real events.⁷ The remarkable persistence of such practices may be due, at least in part, to the fact that they are followed relatively promptly by anxiety-reduction, whereas their experienced futility at the reality level may come many hours or days or even months later.⁸ The persistence of certain forms of 'unrealistic' anxiety-reinforced behavior may also be due to the fact that in most societies there seem always to be some individuals who are able and ready to derive an easy living by fostering beliefs on the part of others in 'unrealistic' dangers. For the common man protection against such 'dangers' consists of whatever type of behavior the bogey-makers choose to say is 'safe' (and which furthers their own interests).

Yet other forms of 'unrealistic' anxiety-reinforced behavior are to be observed in the symptomatic acts of the psychoneuroses. According to Freud, anxiety is in fact 'the fundamental phenomenon and the central problem of neurosis' (3, p. 111). He further says:

"Since we have reduced the development of anxiety to a response to situations of danger, we shall prefer to say that the symptoms are created in order to remove or rescue the ego from the situation of danger. . . . We can also say, in supplement to this, that the development of anxiety induces symptom formation—nay more, it is a *sine qua non* thereof, for if the ego did not forcibly arouse the pleasure-pain mechanism through the development of anxiety, it would not acquire the power to put a stop to the danger-threatening process elaborated in the id" (3, pp. 112-113).

⁷ Under some circumstances, e.g., when warriors are preparing for battle, malevolent incantations or similar anxiety-reducing magical procedures may, of course, be objectively efficacious, not, to be sure, in the supposed magical way, but in that they alter human conduct in crucial life situations (i.e., make the warriors bolder and better fighters).

⁸ Cf. Hull's concept of the 'goal gradient' (4).

Willoughby (12), in a scholarly, well-documented paper, has previously stressed the similarity of magical rites (including religion) and neurotic symptoms and has shown that both types of behavior spring from the common propensity of human beings to deal with their anxieties unrealistically, *i.e.*, by means which diminish emotional discomfort but do not adaptively alter external realities. This excellent study has, in the present writer's opinion, only one important weakness: it takes as its point of departure what Freud has called his "first theory" of anxiety formation (1894), which he subsequently abandoned for the one outlined above. In brief, Freud's earlier supposition was that anxiety arose whenever a strong organic drive or impulse was prevented from discharging through its accustomed motor outlets. According to this view, inhibition was the primary state, anxiety the resultant. In all his more recent writings, on the other hand, Freud takes the position, here also adopted, that anxiety (as a reaction to a "danger signal") is primal and that inhibition, of anxiety-arousing, danger-producing impulses,⁹ is a consequence (3). Reaction mechanisms (magic, symptoms, etc.) that contribute to this end tend, for reasons already given, to be reinforced and perpetuated. Willoughby's analysis is not of necessity predicated upon Freud's original view of anxiety formation and would seem to gain rather than lose cogency if based instead upon his more recent formulations.

Magical and neurotic practices constitute a very perplexing

⁹ One of Freud's most fundamental discoveries, basic to the understanding of reaction-formation, repression, projection, and other neurotic mechanisms, is that organic impulses, even though they are not consciously experienced and identified, may function as 'danger signals' and thereby evoke anxiety. This relatively simple yet frequently misapprehended finding (Freud has himself contributed to the confusion by sometimes speaking as if anxiety is the 'danger signal,' instead of a *reaction* to it) can be readily translated into Pavlovian terminology by saying that an organic need, or drive, which has in the past led to overt behavior that was severely punished will tend upon its recurrence, even at low intensities, to elicit a conditioned pain (anxiety) reaction. Yet, as will be shown in a later paper on the so-called 'experimental neurosis,' Pavlov and his followers have largely ignored this possibility of internal, as well as external, stimuli acquiring "signal" value, *i.e.*, becoming 'conditioned,' and have consequently made apparent mysteries of some laboratory observations which, when viewed more broadly, seem completely intelligible.

and challenging problem from the point of view of traditional psychological theory; but, as Allport (1) has recently pointed out, so also do many other types of human activity that are commonly regarded as both rational and normal. Allport rightly stresses the inadequacies of the conditioned-reflex concept as a comprehensive explanation of learning and personality development in general. He also justly criticizes the view that all human conduct is to be accounted for in terms of trial-and-error striving to eliminate immediately felt organic needs. The plain fact is that much of modern man's most energetic behavior occurs when his organic needs are ostensibly well satisfied. In an attempt to account for this state of affairs, without, on the other hand, falling back on a forthright mentalistic type of approach, Allport elaborates the view, previously advanced by Woodworth, that habits themselves have an on-going character, independent of the motivation that originally brought them into being, and that this type of habit-momentum constitutes a form of self-sustained motivation. Allport calls this the principle of 'functional autonomy' and relies heavily upon it in developing his system of the 'psychology of personality.'

In the estimation of the present writer, 'functional autonomy' is on a par with 'perpetual motion.' Its author clearly perceives an important psychological problem, but it seems unlikely that his is a scientifically tenable solution to it. The position here taken is that human beings (and also other living organs to varying degrees) can be motivated either by organic pressures (needs) that are currently present and felt *or* by the mere anticipation of such pressures and that those habits tend to be acquired and perpetuated (reinforced) which effect a reduction in *either* of these two types of motivation. This view rests upon and is but an extended application of the well-founded law of effect and involves no assumptions that are not empirically verifiable. It has the further advantage that it is consistent with common-sense impressions and practices and at the same time serves as a useful integrational device at the scientific level.

The present analysis of anxiety (anticipation, expectancy) and its role in shaping both 'adaptive' and 'mal-adaptive' behavior in human beings is also consistent with the growing tendency to eliminate the distinction between learning through 'punishment' and learning through 'reward.' The earlier view was that so-called punishment 'stamped out' habits and that reward 'stamped' them in. This distinction now appears to have been spurious and to have depended upon a selectivity of emphasis or interest (7). If an individual is motivated by an internal discomfort or need (produced by his own metabolic processes), and if another individual provides the means of eliminating it, and if, in the process, the first individual acquires new behavior, this is called learning through 'reward.' But if a second individual supplies the need (by inflicting or threatening to inflict some form of discomfort), and if the affected individual supplies the means of eliminating this discomfort (by flight, inactivity, propitiation, compliance, or the like), and if, in the process, this individual acquires new behavior, then this is called learning through 'punishment.' The truth of the matter seems to be that all learning presupposes (i) an increase of motivation (striving) and (ii) a decrease of motivation (success) and that the essential features of the process are much the same, regardless of the specific source of motivation or of the particular circumstances of its elimination.¹⁰

There is, however, one practical consideration to be taken into account. Although learning through 'punishment' does not seem to differ basically from learning through 'reward,' inter-personal relationships are likely to be affected very differently in the two cases. If the method of 'reward' is employed, inter-personal relationships are likely to be made

¹⁰ According to this point of view, old habits are eliminated, not by being 'stamped out' or extracted, as it were, 'by the roots,' but by the functional superimposition of new, more powerful, antagonistic habits (7). Anxiety may thus be said to exercise an 'inhibitory' effect (see foregoing discussion of Freud's 'first theory' of anxiety) upon established behavior trends mainly through its motivation and reinforcement of opposing behavior trends. In this way emphasis falls primarily upon the positive, habit-forming consequences of anxiety and only secondarily and indirectly upon its negative, inhibitory functions.

more 'positive' (*i.e.*, approach tendencies will be strengthened); whereas, if the method of 'punishment' is employed, inter-personal relationships are likely to be made more 'negative' (*i.e.*, avoidance tendencies will be strengthened). From a purely social point of view, it is therefore preferable to employ the method of 'reward,' whenever this is possible; but 'punishment' may have to be resorted to if no *organic* needs are present to be 'rewarded' or if means of rewarding them are not available. Punishment (or the threat of punishment, *i.e.*, anxiety) is particularly convenient in that it can be produced instantly; but this advantage is accompanied by disadvantages which cannot be safely disregarded (8).

Even the practical basis for distinguishing between learning through reward and through punishment just suggested becomes tenuous when one considers the type of situation in which one person withholds from another an expected reward. This, in one sense, is a form of 'punishment,' and yet its effectiveness is based upon the principle of 'reward.' This complicated state of affairs seems especially likely to arise in the parent-child relationship and has implications that have been but slightly explored in stimulus-response terms.

SUMMARY

In contrast to the older view, which held that anxiety (fear) was an instinctive reaction to phylogenetically pre-determined objects or situations, the position here taken is that anxiety is a learned response, occurring to 'signals' (conditioned stimuli) that are premonitory of (*i.e.*, have in the past been followed by) situations of injury or pain (unconditioned stimuli). Anxiety is thus basically anticipatory in nature and has great biological utility in that it adaptively motivates living organisms to deal with (prepare for or flee from) traumatic events in advance of their actual occurrence, thereby diminishing their harmful effects. However, experienced anxiety does not always vary in direct proportion to the objective danger in a given situation, with the result that living organisms, and human beings in particular, show tendencies to behave 'irrationally,' *i.e.*, to have anxiety in

situations that are not dangerous or to have no anxiety in situations that are dangerous. Such a 'disproportionality of affect' may come about for a variety of reasons, and the analysis of these reasons throws light upon such diverse phenomena as magic, superstition, social exploitation, and the psychoneuroses.

Moreover, by positing anxiety as a kind of connecting link between complete wellbeing and active organic discomfort or injury, it is possible to reconcile the fact that much, perhaps most, of the day-to-day behavior of civilized human beings is not prompted by simultaneously active organic drives and the fact that the law of effect (principle of learning through motivation-reduction) is apparently one of the best-established of psychological principles. This is accomplished by assuming (i) that anxiety, *i.e.*, mere anticipation of actual organic need or injury, may effectively motivate human beings and (ii) that reduction of anxiety may serve powerfully to reinforce behavior that brings about such a state of 'relief' or 'security.' Anxiety, although derived from more basic forms of motivation, is thus regarded as functioning in an essentially parallel manner as far as its role as an activating and reinforcing agent is concerned. This analysis is consistent with the common sense view in such matters and does not conflict with any known empirical fact. Finally, it has the advantage of being open to objective investigation and of giving rise to a host of problems that have scarcely been touched experimentally (8).

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THE CONCEPT OF REFLEX RESERVE¹

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B. F. Skinner has recently published an extensive theoretical treatment of behavior (3) together with a large amount of illustrative material based on experiments with the horizontal-bar-pressing response in rats. By means of the 'reflex reserve,' a new and rather radical concept, he has apparently made an extensive integration of a large number of experimental facts of behavior. Extinction, spontaneous recovery, effects of drugs, effects of emotion, drive, and many other known but previously little-related variables fall neatly into an ordered schema. If the predictions which can be made by a combination of the reflex reserve principle with other subsidiary principles proposed by Skinner are borne out by experimental fact, then this principle will be of tremendous value.

Unfortunately, Skinner applies the principle to his own data in only a very general way. As will be shown later, Skinner's basic principles may be expressed very precisely in mathematical terms, and any test of them depends upon a mathematical formulation. In the present paper this mathematical treatment is given and its consequences compared with Skinner's data as far as that is possible, and with some experimental results obtained by the writer, also with a bar-pressing response similar to that used by Skinner.

Two alternative lines of criticism are presented. It is shown that as Skinner treats his own data, two critical assumptions in the theory of the reflex reserve break down. It is indicated, however, that Skinner's treatment of his data is

¹The material contained in this paper formed a part of a dissertation presented in partial fulfilment of the requirements for the degree of Doctor of Philosophy at Yale University. The paper was written in connection with experimental work performed under the auspices of the Institute of Human Relations.

not in accordance with the requirements of his theory. When data similar to Skinner's are examined in a manner satisfying these requirements the theory is again shown to fail. Because of the recency of Skinner's book, and the novelty of his approach, a rather extensive outline of the relevant material is given below.

In summarizing Skinner's system it is first necessary to describe the manner in which his data were recorded, since this differs from conventional methods and is closely related to the concepts included in the system. Skinner avoids the use of averages wherever possible; his system applies chiefly to data obtained from single individuals. The response or 'reflex' studied is defined operationally in terms of its effect upon the environment (depression of the bar) without regard to the particular activities or muscle systems involved in any single occurrence. Only one characteristic of the response is measured: the interval between responses, or 'rate of response.'² Responses are graphed cumulatively against time so that the rate of response at any point is indicated by the slope of the curve. Thus the process of extinction is represented by a negatively accelerated curve, concave downward.

The negative acceleration of the extinction curve suggests that there is a limit to the height of this cumulative curve, and thus to the number of unreinforced responses the animal will make. This limit, called by Skinner the 'reflex reserve', is a basic assumption of the system. This and other critical principles as formulated by the writer are presented below:

1. The reinforcement of a response following its occurrence (conditioning of Type R) builds a *reflex reserve*, this reserve being the total number of times the response will subsequently occur without further reinforcement (granting that the environmental conditions are adequate for its performance).

2. The reflex reserve can be increased or decreased only

² In one chapter, entitled 'The Differentiation of a Response,' Skinner measures and discusses the amplitude or force of a response, but this is not necessarily an integral part of the system of behavior to which the present discussion is relevant.

by operations involving actual elicitation, reinforcement increasing and extinction decreasing the reserve.

3. The strength of the reflex at any point during extinction is proportional to the remaining reserve (*i.e.*, the number of responses yet to be made).

A. The strength of the reflex is measured by the slope of the curve, or more properly by the slope of a smooth envelope curve drawn above and as close as possible to the obtained extinction curve.

4. The proportionality between the strength and the reserve may be changed by a number of factors such as emotion, drive, drugs, etc., these factors having no effect upon the magnitude of the reserve itself.

5. If the extinction is interrupted before the reserve is emptied, the rate of response following resumption (spontaneous recovery) is temporarily determined by an 'immediate reserve,' which represents the discrepancy between the number of responses which occurred up to the beginning of the interruption and the number that would have occurred if there had been no interruption (the height of the original envelope curve extrapolated to the point when the interrupting conditions were removed). When the original envelope is again reached, the rate of response is again determined by the total reflex reserve.

These five principles are the crux of the system which Skinner presents. They are of especial interest because they differ radically from any which have previously been proposed to explain this type of behavior, and because of the apparent success with which Skinner has been able to use them to integrate a large number of experimental findings. The integration is obtained by placing a large number of experimental variables into only two classes, those which affect the *size* of the reflex reserve (principle 2), and those which affect the *proportionality* between the rate of response and the number of responses remaining in the reserve (principle 4).

The major divergence of Skinner's theory from other theories results from the introduction of the notion of 'reflex reserve,' an hypothetical concept which is assumed to have very definite and important effects upon the manifestation

of the response. Unfortunately, the very nature of the reflex reserve and its relationship to other variables make its direct measurement impossible. A zero rate of response for a given period does not mean that the animal will not respond later, or under changed conditions, and thus one can never be sure that the number of responses which have occurred have emptied the reserve.

However, other assumptions of the system permit a determination of the magnitude of the reflex reserve from the characteristics of the extinction curve before the reserve is emptied (namely, principle 3, above). Skinner says:

In one sense the reserve is a hypothetical entity. It is a convenient way of representing a particular relation that obtains between the activity of a reflex and its subsequent strength. But I shall later show in detail that a reserve is clearly exhibited in all its relevant properties during the process that exhausts it and that the momentary strength is proportional to the reserve and therefore an available direct measure. The reserve is consequently very near to being directly treated experimentally, although no local or physiological properties are assigned to it (3, p. 26).

The measurement of the reserve by means of a segment of the extinction curve is made possible through the third principle given above, which states that the rate of response, or slope of the envelope curve is proportional to the remaining reserve. Skinner presents a family of curves which satisfy these requirements (3, p. 381) and states that they approximate experimental curves very closely (p. 380). However, he does not present his data in such a form that they can be compared with the theoretical curves, and does not give the form of the equation by which the curves were plotted. This is unfortunate since the integrative value of the system depends upon the correspondence of the extinction curve envelopes to the shape of the mathematical curves. If the experimentally obtained extinction curve can not be described by the required theoretical equation, then the magnitude of the reserve can not be determined, and it becomes impossible

to determine whether the effects of such factors as fatigue, emotion, drive, etc., are to change the proportionality between reserve and rate (principle 3). We are limited to the statement that the effect of these variables is to change the rate of response or the total number of responses—a statement which could have been made before Skinner's system appeared.

The form of the equation which satisfies the requirement of 'proportionality between rate of response and the remaining reserve' is as follows:

$$r = R(1 - e^{-ct}),$$

where r is the number of responses, R is the asymptote (or reflex reserve), t is the time since the first response, and c is a constant. This is apparently the form used by Skinner to plot his theoretical curves; it permitted the duplication of the curves which he presents graphically (3, p. 381) within the limits of error of measurement.

In spite of the tremendous amount of relevant data which he has accumulated, Skinner does not report any instances in which he has determined whether this equation actually describes an envelope extinction curve. This is particularly unfortunate since he did not publish his data in such a form that others could do so. In one chapter (3, Chapter Three) he has fitted a number of *logarithmic* curves as envelopes to experimental extinction curves. This is itself presumptive evidence that the rate of response is not proportional to the remaining reserve, since the log curve does not have this property. In fact, if this curve proved to fit the curve of an extinction continued indefinitely, it would disprove the reflex reserve concept itself, since the log curve has no limiting value. However, since actual behavioral data is never described perfectly by a simple mathematical formula, and the present case is no exception, it is quite possible that the exponential equation given above could also describe the empirical curves.

Since Skinner's data were not presented in such a form that they could be treated mathematically, the writer has analyzed

some of his own data obtained in a situation similar to Skinner's (1, 2).³ Twelve arbitrarily selected extinction curves, obtained from rats with the horizontal-bar-pressing response, were plotted cumulatively against time. Envelope curves⁴ of the form: $r = c \log (t + 1)$ and $r = R(1 - e^{-ct})$ were fitted to each of the experimental curves. The above form of the log curve was substituted for the one which Skinner mentions ($N = k \log t$), (3, p. 88) since it has the presumably required property of passing through the origin, and since the change has very little effect for large values of t .

In fitting these curves, from 3 to 6 points on each of the 12 extinction curves were selected as being 'high' or 'outside,' and both mathematical curves were fitted to these points. In order to preserve the quality of an envelope (see footnote 4) the deviations of the calculated r 's below the corresponding experimental r 's were kept at a minimum. A strict definition of 'envelope curve' would presumably prohibit any deviation of the mathematical curve *below* the experimental curve. However, when strict adherence to this requirement at a given point would have greatly increased the total deviation of the selected experimental r 's from the corresponding r 's on the mathematical curve, the mathematical curve was permitted to fall slightly below the experimental one at this point. The extent to which this was done may be seen from Fig. 1. It should be emphasized that the effect of this flexibility of treatment was always in a direction which favored the theory, since it tended to *increase* the value of R (see discussion below).

The mean A.D. of the calculated r 's from the experimental r 's was approximately three times as great for the log curves as for the theoretically required exponential curves. Six of the twelve extinction curves, together with the envelopes calculated by means of the exponential equations, are given in

³ These papers contain the details of the procedure used but do not present the data in the form in which it is here analyzed.

⁴ The term 'envelope curve' is Skinner's, and while it has not been assigned any specific mathematical qualities, its meaning is fairly clear from the context of Skinner's discussion. For purposes of the present treatment, an envelope is taken to be one which is *above* and as *close* to the actual extinction curve as possible.

Fig. 1. This equation permits a reasonably accurate fit to the extinction curves obtained in the writer's experiment,

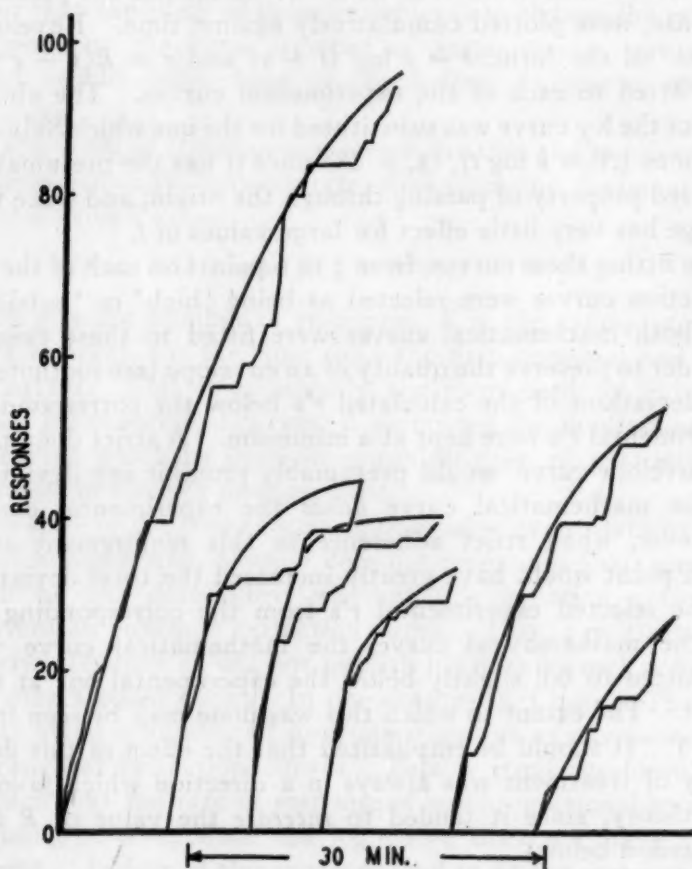


FIG. 1. Cumulative curves of the first extinction for each of the even-numbered animals in the 185-minute group in (2). The smooth curves in each case were fitted as envelopes, using the equation: $r = R(1 - e^{-et})$.

thus far conforming to the requirements of Skinner's principles.⁵

⁵ It should be mentioned, however, that although the exponential equation apparently permits a better fit than the logarithmic equation here, this is not necessarily always the case. The extinction curves obtained by the writer were terminated by a criterion such that their average duration was approximately 15 minutes, while Skinner's, on the other hand, were usually continued for an hour or more. A careful in-

Having fitted the exponential equation: $r = R(1 - e^{-ct})$ as an envelope to the first extinction curves of 12 animals, we have necessarily determined the value of R for each of these animals. R , it will be recalled, is the asymptote of the envelope curve, and as such determines the rate of response, since the rate is proportional to $(R - r)$ or the remaining reserve. R is the reflex reserve, the total number of unreinforced responses the animal will make. The hypothesis requires that if the animals receive no more reinforcements, each animal should not make a total number of responses greater than the value of R in the equation fitted to his first extinction curve.

The above definite implication of Skinner's principles happened to be one which could be checked by data obtained in one of the writer's experiments (2). The 12 animals for whom the R was computed, comprised a group given five successive extinctions with an interval of three hours between extinctions (2). R 's were computed from the curves obtained in the first extinction for each rat. According to the hypothesis, no animal should make a total number of responses greater than the value of R computed from its first extinction curve. Yet 11 of the 12 animals made more responses in the five extinctions than was indicated by their respective R 's. The differences between the distribution of R 's and the total number of responses made, when computed by the χ^2 method, were highly significant. Evidently the rate of response is not proportional to the number of responses yet to be made, since it is proportional to $(R - r)$, and R is not the total number of responses which a given animal will make. Principle 3 does not correspond to experimental fact.

Since the reflex reserve is not susceptible to direct measurement and the proportionality principle, by means of which the reserve was to have been measured indirectly, was not verified

specification of Skinner's curves and a comparison of them with the possible forms of the exponential curve suggests that these longer curves could not be fitted by the exponential equation. The curves plotted from this equation are relatively inflexible and it would not be possible to find one which would combine the steep initial slope with the gradual falling off which most of Skinner's curves show.

by experimental data, we must conclude that the reflex reserve is not determinable (unless or until another indirect measure is proposed and verified). As a result of this failure of the third principle, the second and fourth principles also become unverifiable, since they state relationships between the size of the reflex reserve and other variables. The first principle, of course, which states that reinforcement creates a reserve, is meaningless until further propositions stating relationships between the reflex reserve and measurable variables are established.

If the extrapolation of the curve fitted to the first extinction is considered to describe the course of a continuation of that extinction, then the fifth principle is also disproved, since following the recovery intervals the animals make more responses than they would have made if the extinction had not been interrupted. (It is clear, of course, that if the asymptote of the curve (R) is exceeded, the curve itself is also exceeded.)

It might be suggested that the method by which the disproof of the fifth principle is obtained is itself questionable, since it involves the extrapolation of psychological data. However, this method is suggested by Skinner, who uses it himself in his discussion of spontaneous recovery. It is a necessity as the system is formulated since his use of an envelope curve drawn *above* the obtained extinction curve precludes the use of averages, and thus to a certain extent, of control groups. In a discussion of extinction he stresses this point, as follows:

Because of the cyclic fluctuation and its variable wave length, it is useless to appeal to averages. The deviations are all in one direction if the present interpretation is correct. A smooth curve could doubtless be obtained by averaging a large number of cases but it would not represent the envelope approached by each curve separately (3, p. 78).

Since it is the envelope curve which is involved in the theory, predictions must be made from the individual record, and this requires extrapolation.

CONCLUSIONS

The concept of reflex reserve has been considered in relation to experimental data to which it is relevant. The principle postulating a proportionality between the rate of response during extinction and the remaining reserve was not verified. As a result, the size of the reflex reserve was found to be unmeasurable and statements expressing a relationship between the size of the reserve and other variables are unverifiable.

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THE SIMPLE RESEAU PATTERN

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INTRODUCTION

When two systems of equally-spaced parallel lines intersect, a series of diamond-shaped figures is produced. When the angle of intersection is small, an interesting pattern, part actual and part illusory, is produced. This, resembling a series of dark bands bisecting the larger angle of intersection, is the simplest form of *Reseau Pattern* (Fig. 1).

Elimination of patterns of this type in two, three, and four color half-tone printing was, until recently, a matter of 'cut and try,' with the many unexpected failures raising the cost of high grade color printing to almost prohibitive heights. Recently-published papers by Bull, Cox, and Hallam¹ have indicated a practical solution for this engraver's problem. Cox and Hallam suggest that much pattern trouble is caused by the use of half-tone screens in which the two sets of rulings are not *exactly* at right angles, and by lack of facilities for rotating screens through *exactly* a predetermined angle (30 or 60 degrees) between 'shots.' With actual examples, the writers demonstrate that when the angle between intersecting systems of lines (or rows of elements) is greatest, the pattern produced is least.

Theoretical solutions of Reseau Pattern problems are quite complex, involving not only mathematics but psychology. In the case of a pattern produced by the superimposition of three half-tone impressions, each rotated through a given angle with respect to the one directly 'under' it (Fig. 1, D) the calculations alone become both involved and

¹ A. J. Bull, *Photo. J.*, 1938, 78, pp. 130-132. R. S. Cox & C. D. Hallam, *Photo. J.*, 1938, 78, pp. 24-27. R. S. Cox & C. D. Hallam, *Photo. J.*, 1938, 78, pp. 584-586.

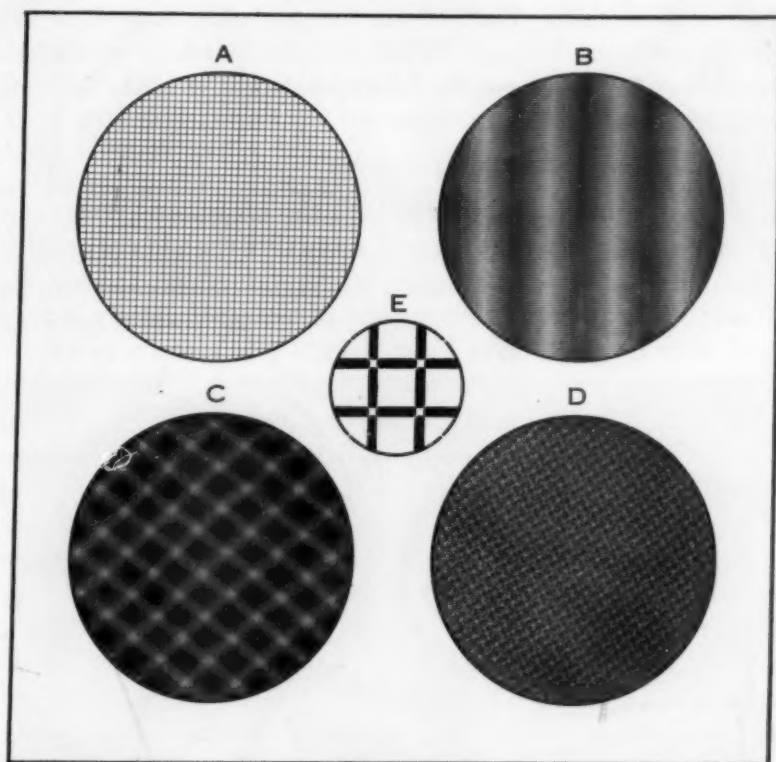


FIG. 1. Various Reseau Patterns

A. A reseau, or network of lines. This pattern, where the two systems of lines intersect at right angles, has a minimum of illusory qualities, only the 'lightening of intersections' illusion, shown in greatly exaggerated form in *E*, being present.

B. A more complicated Reseau Pattern, produced by the intersection of two systems of equally-spaced parallel lines at an acute angle. Because of numerous optical illusions, the dark vertical bands appear darker than they actually are, and the light bands between them lighter.

C. Plaid pattern produced when two reseaus (in this case white lines on a black field) are superimposed, with a slight rotation of the 'upper' pattern with respect to the lower.

D. Three reseaus superimposed give this pattern, the various elements being partly real and partly illusory. This is similar to the pattern produced when three half-tone impressions are superimposed in color printing.

voluminous, and the psychological considerations of the illusions produced 'go off the end of the known.'

Deliberate creation of simple Reseau Patterns for illustrative or decorative purposes has been attempted with varying

degrees of success. Further use has probably been retarded by the apparent uncontrollability of the patterns produced. Reproduction of very small Reseau patterns is made difficult by diffraction troubles.

ILLUSIONS

Probably in no practical field is the oft-quoted statement "things are not what they seem" more true than in the graphic arts. In almost every form of reproduction, shades are illusory, being actually composed of series of closely-spaced dark dots or lines on a light field. The human eye-brain system integrates these as tones of solid gray. Likewise, the 'black' of a half-tone reproduction, composed of dark dots large in comparison to the light area between them, is interpreted as solid black, even though it may reflect 30 per cent of the light falling upon it; while the 'white' of the same figure, actually composed of dark dots, small in relation to the light area between them, is interpreted as 'pure' white, although it may actually reflect less than 70 per cent of the light falling upon it. Here, the eye-brain system interprets the darkest area as black, and the lightest area as white. Similar exaggerations of contrast may be produced photographically, particularly when engraver's films, having an 'all or nothing' characteristic, are used.

All Reseau Patterns, regardless of the angle between the systems of lines, are partially illusory. Unfortunately, the illusion varies from observer to observer, and even from time to time with the same observer, so that no valid quantitative analysis of the illusions can be made, nor can an illusion equally effective for all observers be produced.

In the right-angled Reseau (Fig. 1, *A*), the illusions are fewest, only the 'lightening of intersections' illusion, shown in greatly exaggerated form in Fig. 1, *E*, being commonly reported. Various forms of this illusion occur in Reseau Patterns of all angles, as a result of irradiation.² These

² "Irradiation has been defined as the lateral diffusion of nervous stimuli beyond the actual stimulus." M. Luckiesh, *Visual Illusions*, New York: D. Van Nostrand, 1922, p. 120.

irradiation effects, almost identical to photographic flaws produced by halation, and quite similar to those produced by improper focus coupled with great exaggeration of contrast, are partly responsible for the success of the half-tone method of reproduction.

In the oblique, or acute-angled Reseau (Fig. 1, *B*), a number of illusions are present. The perceived figure is substantially identical with that produced when a system of equally-spaced parallel dark bands on a light field is photographed slightly out of focus, as in Fig. 2.

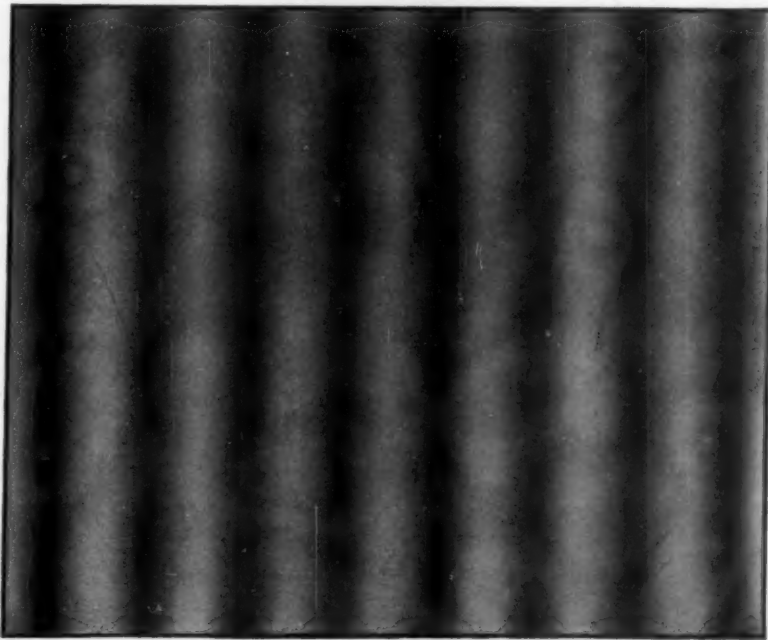


FIG. 2. Pattern produced when a system of equally-spaced parallel black bands on a white field is photographed somewhat out of focus. Note the superficial similarity of this pattern to that in Fig. 1, *B*.

When a large acute-angled Reseau, composed of lines of considerable width (Fig. 3, *A*), is observed, two general types of illusory patterns are produced. One, in which the obtuse intersections are thinned and the acute intersections filled with black (the "ink-drop illusion") is quite common (Fig.

3, *B*). Here, irradiation is the major illusion-producing factor. This illusion may be duplicated by photographing the Reseau of Fig. 3, *A*, with the camera *slightly* out of focus, as is shown in Fig. 4, *B*.

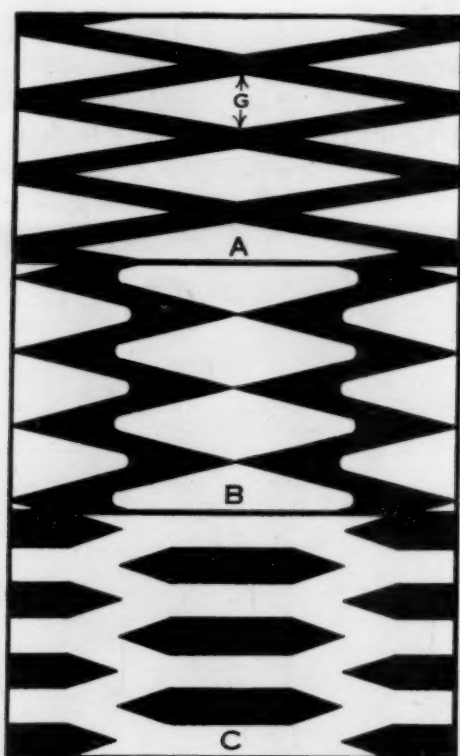


FIG. 3. Illusions produced by a large, acute-angled Reseau.

A. The original Reseau.

B. Sketch of one common illusory pattern perceived when *A* is viewed casually.

C. Sketch of a second common illusory pattern perceived when *A* is viewed casually.

The second illusory pattern commonly observed (Fig. 3, *C*) somewhat resembles a negative of the original figure, with the dark areas translated vertically the vertical distance between intersections of the lines, or horizontally a comparable distance. This figure may be approximately duplicated by photographing the Reseau of Fig. 3, *A*, with the camera

considerably out of focus, as is shown in Fig. 4, *C*. Still greater defocussing breaks the original figure (Fig. 3, *A*) up into a pattern of 14 elliptical dots, but no similar illusion has been observed by, or reported to, the writer.

These illusory figures may be attributed to fatigue of accommodation, which produces, in the eye, exactly the same effects as are produced by defocussing a camera. The second illusion, shown in Figs. 3, *C* and 4, *C*, may also be explained

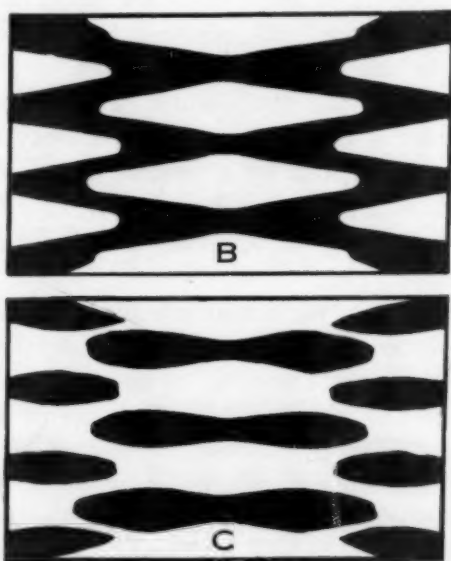


FIG. 4. Figures produced when Fig. 3, *A*, is photographed with the camera out of focus.

B. Figure similar to Fig. 3, *B*, produced by slight defocussing.

C. Figure similar to Fig. 3, *C*, produced by somewhat greater defocussing.

as after-images of the original, although, in most cases, the illusory figure does not gradually fade away, as does a normal after-image.

When the original figure (Fig. 3, *A*) is relatively large (*i.e.*, *G*, in Fig. 3, *A*, is of the order of one inch), and the total black area is approximately equal to the white, either illusory figure may appear at first glance. Later, the perceived figure will suddenly 'snap over' from one type of illusion to the

other. If the distance between the eye and the figure viewed is varied, one or the other of the illusions can usually be brought into predominance. In general, although not in every instance, illusion *B* (Fig. 3) predominates when the Reseau is far from the eye, while *C* becomes more important when the figure is near the eye.

Occasionally, it is noted that one eye perceives one illusory figure, and the other eye a second. The conflict occurring under this condition, if both eyes are used, is severe, and results in nervous irritation, followed, if observation is persisted in, by a severe headache and nausea.

In general, when the black-white ratio is about 1/1, and the original figure is fairly large, either illusory figure can be perceived, not at will, but by a process of 'inverse volition': *i.e.*, if the original is looked at intently, in an effort to secure illusions of *B* type (Fig. 3), an illusion of *C* type will appear, and vice-versa.

In some instances, particularly when the eye is fatigued, the illusions perceived will alternate rapidly from one type to another, with some 'creeping' of the patterns in the peripheral areas of the visual field. This seems to be closely related to the 'drifting' of visual focus so often noted by photographers and microscopists.

When complicated Reseau Patterns are viewed under almost any conditions, some 'creeping' or 'whirling' of the pattern elements is perceived in the peripheral areas of the visual field. It is here suggested that some of the 'offensive' color prints of recent years, criticized for their glaring or irritating hues, actually are chromatically correct, but produce conflicting illusions, as a result of screen pattern, similar to those perceived when an acute-angled Reseau is viewed with a tired eye. Some of these prints, when viewed or reproduced in monochrome, are still offensive, plainly indicating that pattern, and not color, is the cause of the trouble. In a few instances, the offending impression may be detected by 'dropping out' each impression in turn, by means of suitable viewing filters, until the trouble disappears.

As the size of the figure is reduced, all other factors re-

maintaining substantially the same, illusions of type *C* (Fig. 3) occur with lesser frequency, and when the figure is quite small, (*G*, in Fig. 3, *A* is less than 1/16 inch) only illusions of type *B* can be perceived at normal viewing distance. If the size of the figure is still further reduced, the thin obtuse intersections in *B* (Fig. 3) are apparently replaced by open channels connecting the larger white areas, while the acute intersections seem to be more completely filled with black. Thus, when the size of an element of a simple Reseau Pattern, originally resembling Fig. 3, *A*, is reduced, the figure perceived tends to resemble Fig. 1, *B*, and if the reduction is carried far enough, the figure perceived will be substantially identical with Fig. 2.

It is apparent, from experiments, that the relative, rather than the absolute, size of the original is an important factor in the production of the illusory features of any Reseau Pattern. The ratio of illusory to actual features perceived seems to bear an inverse relation to the solid angle subtended at the eye by a single element of the pattern, and hence to the number of rods and cones stimulated by the retinal image of that pattern element. Here, also, is a clue to the great individual variation in the illusions perceived—no two human eyes are exactly alike. Similarly, the increase in the illusory features of any Reseau as the angle between the systems of elements is decreased may be explained. On this basis, it will be noted that, from the standpoint of illusion-minimization, the photoengraver's practice of separating the screens of the three darker printers in four-color half-tone work by 30 degrees (the maximum possible angle) is sound. The yellow impression, substantially a right-angled white Reseau on a yellow field, produces no perceptible illusions in combination with the red, blue, and black impressions.

The inconstancy of the illusory features of a Reseau pattern for a single observer is partly influenced by his physical state. Optical fatigue tends to increase the magnitude of the illusions, to make them less stable, and to increase the observer's nervous irritation. True stimulants decrease the magnitude of the illusory features, while hypnotics increase

their effectiveness. Certain common drugs introduce colored fringes to complicate the illusions. Quinine, for example, when taken in large doses (40 gr.), produces a red fringe in the whites of a Reseau Pattern, and a greenish fringe in the blacks. Hunger seems to increase the magnitude of the illusions, and to make them less stable. A change of altitude of 5,000 feet (either way) causes a temporary increase in the illusions, which lasts until partial, or 'first,' acclimation is attained. Small amounts of alcohol act like a stimulant; in larger doses it causes the same effects as a hypnotic. Its effects vary tremendously from observer to observer, and widely from day to day with the same observer.

If a Reseau Pattern is viewed through filters of different colors, the illusions are of least magnitude, during 'day' vision, for the average observer, when a yellow filter, such as a Wratten G, is used. During 'night' vision, the spectral range of greatest visual acuity, and hence of minimum illusions, is shifted toward the red end of the spectrum. The writer finds that the illusions are least, during night vision, when the pattern is viewed through a Wratten 88-A (infrared) filter, which, to day vision, is a piece of black glass. The spectral range of greatest visual acuity during day vision varies little from observer to observer. Night vision, however, seems much less constant, but in every instance is shifted toward the red end of the spectrum.

No relation has been found between general intelligence and the reported magnitude of the illusions seen, nor between I.Q. and the type of illusion first seen. Observers having visual defects become irritated more quickly on perceiving Reseau illusions than those having good eyes or properly fitted glasses. Those with uncorrected astigmatism report that patterns similar to Fig. 1, A, 'lean' somewhat, or that the figure is 'out of round.' Two observers insisted that Fig. 1-A was an ellipse, with the minor axis vertical, and persisted in the belief even when it was rotated through 90 degrees!

Personality-experience factors have a definite influence on the types of illusion reported, and may have an effect on those perceived. Although too few observers were tested to

justify definite statements, it appears that those whose work requires only the use of black and white, such as draftsmen, engineers, etc., are 'blind' to half-tones, and have a more pronounced 'all or nothing' perceptual characteristic than those who must deal with half-tones, such as biologists, artists, etc. This trait is probably related to the acquired monochrome perception common among photographers who work continually with black-and-white sensitive materials.

MATHEMATICAL CONSIDERATIONS

The mathematical consideration of the real features of a complex Reseau Pattern, similar to that produced when three half-tone impressions are superimposed at the 'wrong' angles, is so involved that it becomes a fit subject for a mathematical treatise. The real features of the simpler Reseau Patterns, however—those patterns produced by the intersection of only two systems of parallel lines—may be treated by simple, straightforward mathematical methods, involving nothing more complicated than simple trigonometry.

The simple Reseau of Fig. 1-B is composed of many identical diamond-shaped figures. If one of these is enlarged, a figure similar to Fig. 5 results. From this the real properties

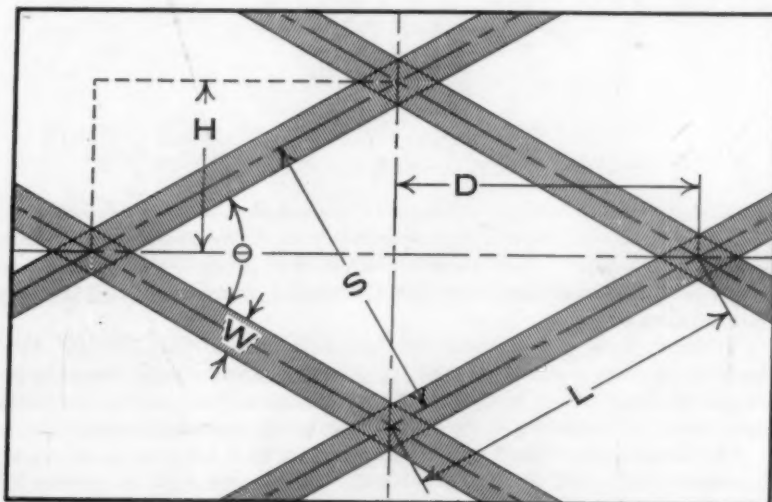


FIG. 5. The elemental diamond.

of the pattern can be determined by simple calculation. This figure will be referred to as the *elemental diamond*.

The elemental diamond is an equilateral parallelogram, or rhombus.³ Hence, the diagonals are bisectors of the interior angles, and perpendicular bisectors of each other.⁴ In this figure, the distance between the centers of the bounding lines, the widths of the lines, and the angle between the systems of lines, are either known or can be determined by simple measurement. For convenience, the elemental diamond is so oriented that its longer diagonal is horizontal.

In Fig. 5 W is the width of any line (known),

S is the distance between centers of lines (known),

θ is the acute angle between line systems (known),

L is the length of any side of the elemental diamond,

D is the horizontal distance between successive intersections,

H is the vertical distance between successive intersections.

Then:

$$S/L = \sin \theta,$$

$$L = S \csc \theta,$$

$$D/L = \cos \theta/2,$$

$$D = L \cos \theta/2 = S \csc \theta \cos \theta/2 = S/2 \csc \theta/2.^5$$

³ Opposite sides of this figure are parallel by construction, hence opposite angles are equal by the transversal theorem of plane geometry (G. A. Wentworth & D. E. Smith, *Plane and Solid Geometry*. New York: Ginn & Co., 1899, p. 50). As the perpendicular distance between pairs of sides is the same, the length L of any side (Fig. 5) is $S \csc \theta$. Hence, all sides are equal.

⁴ "The diagonals of a parallelogram bisect each other" (*Op. cit.*, p. 64). As all sides of the elemental diamond are equal (3), any two adjacent triangles formed by two sides and the diagonals may be shown to be congruent (three sides), and the four central angles, about the intersection of the diagonals, are equal, hence right angles.

⁵ As the conversion from $S \csc \theta \cos \theta/2$ to $S/2 \csc \theta/2$ is not given in the average trigonometry text, it will be given in full here. The conversion can be justified if it can be shown that $S \csc \theta \cos \theta/2 = S/2 \csc \theta/2$ is a true equation. Dividing both sides by

Similarly,

$$H = L \sin \theta/2 = S/2 \sec \theta/2.$$

The elemental diamond, as should be obvious from the figure, is bounded by the centers of the lines forming its sides. The area of any one of the four right triangles forming the figure is $DH/2$, and that of the whole figure is $2DH$. Expressed in terms of S and θ , the area A is given by the equation:

$$A = 2(S/2 \csc \theta/2)(S/2 \sec \theta/2) = S^2/2 \csc \theta/2 \sec \theta/2 \\ = S^2 \csc \theta.$$

If the lines have a finite width, W , the white area inside the elemental diamond may be calculated from the above formula by substituting $(S - W)$ for S . The dark area may be determined by subtracting the white area from the whole area, and is

$$S^2 - (S - W)^2 \csc \theta \text{ or } (2SW - W^2) \csc \theta$$

The ratio of dark area to total area, then, is

$$\frac{\text{dark}}{\text{total}} = \frac{(2SW - W^2) \csc \theta}{S^2 \csc \theta} = \frac{2SW - W^2}{S^2} \\ \text{(note that this ratio is independent of } \theta \text{).}$$

The ratio of white area to total area, similarly, is

$$\frac{\text{white}}{\text{total}} = \frac{(S - W)^2 \csc \theta}{S^2 \csc \theta} = \left(\frac{S - W}{S}\right)^2 \\ \text{(this ratio, also, is independent of } \theta \text{).}$$

Finally, the ratio of dark area to white area is

$$\frac{\text{dark}}{\text{white}} = \frac{2SW - W^2}{(S - W)^2}.$$

These ratios give the relations between white and dark for any area containing N whole elemental diamonds. Along S and changing to direct functions of θ and $\theta/2$:

$$\frac{\cos \theta/2}{\sin \theta} = \frac{1}{2 \sin \theta/2}.$$

Multiplying both sides by $2 \sin \theta/2$ gives

$$\frac{2 \sin \theta/2 \cos \theta/2}{\sin \theta} = 1.$$

As $2 \sin \phi \cos \phi = \sin 2\phi$, the equation is a true identity, and the conversion is justified.

any ordinate, however, these ratios do not hold. An ordinate through the intersections would pass through a zone where the dark-white ratio was least and less than indicated by the calculated ratios, while one taken at any distance greater than X (Fig. 6) from an intersection would (under most

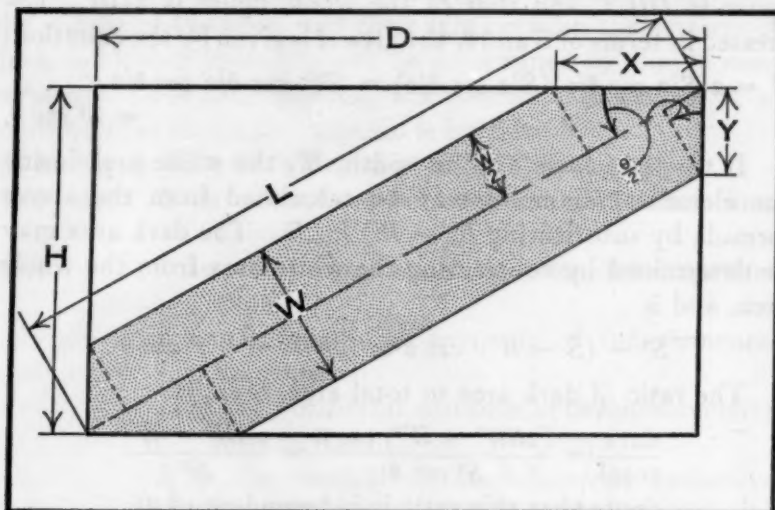


FIG. 6. The rectangle DH of Fig. 5, enlarged.

conditions) pass through a zone where the dark-white ratio was greatest and greater than indicated by calculated ratios.

For convenience, the diagonal of the rectangle HD (Fig. 5) is used in the consideration of the variations in dark-white ratio along ordinates. This is drawn to large scale in Fig. 6. It should be obvious that the dark-white ratio has the smallest numerical value at intersections, and the greatest in a zone between them.

At intersections:

$$\frac{\text{dark}}{\text{white}} = \frac{Y}{H - Y} = \frac{W/2 \sec \theta/2}{(S/2 - W/2) \sec \theta/2} = \frac{W}{S - W}.$$

Between intersections, the ratio approaches

$$\frac{\text{dark}}{\text{white}} = \frac{2Y}{H - 2Y} = \frac{W \sec \theta/2}{(S/2 - W) \sec \theta/2} = \frac{2W}{S - 2W}.$$

and reaches it when $(H - 2Y)$ is positive or zero, as is usually the case when positive Ben Day screens are used to produce the pattern. Hence, when $(H - 2Y)$ is not negative, the variation in the black is in the ratio of two to one. The critical value $(H - 2Y)$ may be expressed in terms of S and W as follows:

Since $(H - 2Y)$ must not be negative, $H/2Y$ must not be less than one.

$\frac{H}{2Y} = \frac{S/2 \sec \theta/2}{2(W/2 \sec \theta/2)} = \frac{S}{2W}$ and $(S - 2W)$ must not be negative.

Geometrically expressed, this means that when the clear space $(S - W)$ between lines is not less than the width, W , of a line, the length of the dark portion of an ordinate midway between intersections will be twice that of an ordinate through an intersection. When the white space between lines is smaller than the width of a line, some of the ordinates between intersections are entirely black, and their black portions, within the rectangle DH , vary in length from Y to H , instead of from Y to $2Y$.

CONCLUSIONS

It is apparent, from mathematical considerations, that the real dark-white relation in a simple Reseau Pattern is a function only of the widths of the lines forming it, and of the spacing of those lines.

It is equally apparent, from common observations, that that figure perceived bears little relation to the real pattern. A typical example is a pattern similar to Fig. 1-B, where at intersections the black-white ratio is one-third and in the zone midway between them it is one-half. Here, with a variation in black of 1 to 2, the average observer reports that the figure consists of vertical bands alternately 'pure white' and 'dead black,' with a 'little running at the edges,' a rather good description of Fig. 2! Experiments show that the perceived figure is altered by changes in the size of the elements relative to the observer's eye, and by changes in the observer's physical condition.

At present, the real features of simple patterns of this type may be determined with any desired accuracy, either by direct measurement or simple calculation. The illusory features, however, being a function not only of the pattern, but of its relation to the observer, and of the observer's personal equation, cannot be so easily determined.

In general, when all real features of the pattern remain the same (*i.e.*, the dark-light ratio is unchanged), the illusory features increase as the size of the elemental diamond is decreased, and as the angle between the component line systems becomes smaller.

Hence, when a pattern in which the real and perceived values are substantially the same is desired, the elemental diamonds must be large, and as nearly rectangular as possible. This minimizes the importance of the optical illusions. When the real values are to be exaggerated, the elemental diamonds should be small, and their vertical diagonal as small as possible relative to their horizontal diagonal. This makes θ small. In this way, the optical illusions will approach their greatest effectiveness.⁶

The final solution of the numerous 'pattern' problems is still far in the future. The color photoengraver has found a practical solution for his problems. Most of the mathematical and psychological problems raised by the various 'pattern troubles' remain to be solved. Of these, the psychological problems seem the most difficult.

It is hoped that this brief discussion of the problems and the brief analysis of the simple Reseau Pattern will stimulate further research in this field.

[Received August 4, 1939]

⁶ Another series of illusions may be introduced if the Reseau Pattern is reproduced, as a result of limitations in the resolving power of lenses, the unavoidable imperfections in engravings, and the capillary properties of the paper on which the pattern is printed. These, in ordinary work, while tending to enhance the illusions, are definitely of minor importance.

REPLY TO DR. GARRETT

BY KURT LEWIN

The State University of Iowa

I would like to ask Dr. Garrett whether he knows of any psychological school which avoids in its descriptions concepts like approach and withdrawal, like ingroup and outgroup, like social position and change in position, like leaving a group and entering a group. He will agree, I suppose, that those terms imply the concept of direction, distance or position. That means spatial concepts. I suppose he will agree too that these distances and directions usually cannot be defined as physical distances and directions, particularly in cases of social actions, in cases of approach and withdrawal by means of language, etc. The spatial character of these concepts is not weakened by calling them 'spatial analogies.' Therefore, either scientific psychology has to cease using such descriptions because they are statements without definite meaning, or it will have to get clear about the kind of geometry which underlies such statements. To abandon these concepts is hardly possible because all psychological schools make use of them. L. B. Murphy's insightful analysis of sympathy which Garrett mentioned as an example of 'genuine psychological concepts without benefit of spatial analogy' is certainly full of such concepts, and one of her main findings, that of 'areas of security and insecurity' (5, p. 299) is expressed in spatial terms. Indeed, problems of psychological 'regions,' of 'position' and change of position or direction seem to become increasingly important in all fields of human and animal psychology.

Of course the problems of psychology cannot be solved 'simply by the use of topology,' no more than the problems of physics can be solved simply by applying Euclidean or Riemannian geometry to them. None of the research work

related to my theory has ever attempted this. One has to make use of the so-called dynamic concepts or constructs (such as tension and drive) and has to formulate specific theories concerning conditions and effect. However, it seems to become more and more evident that any such psychological theory, no matter of what school, presupposes certain spatial concepts: The formula that behavior is a function of the person and his environment seems to be accepted generally. This implies a theoretical field proposition, at least fundamentally. Even the conditioned reflex approach takes more and more into account the fact that all psychological behavior depends upon 'a multitude of coexisting facts' ('stimulus pattern,' Hull), that is to say, a field which has to be represented as a space. Further, all psychological schools use as a basic dynamic concept a construct which has the character of a vector (called excitatory tendency, drive, libido, psychological force). This again presupposes psychological directions and, therefore, a space in which these directions are defined.

The problem of space in psychology, therefore, has to be settled somehow independent of schools, and I think I have shown that the spatial descriptions in psychology have definite mathematical meaning if one interprets them not by Euclidean geometry but by topological and hodological geometry. I do not feel that Dr. Garrett gives full consideration to the purely formal character of geometrical concepts, the application of which is not at all restricted to physical space. He quotes the statement of Heidebreder that the topological diagrams are 'misleading if interpreted in terms of the usual metrical geometry.' Dr. Garrett infers from this statement that "you are not to take topological diagrams as accurate geometry." However, topology is certainly as accurate and logically stringent as any other geometry although it is a non-metrical geometry.¹ A similar misunderstanding seems to exist in

¹ By the way, one can hardly say that "topology is not well known even to American mathematicians." American mathematicians (Veblen, Lefschitz, Alexander) have always led in topology and today interest in this field is most common among American mathematicians.

regard to the mathematical term 'space' as distinguished from physical space, in spite of my elaboration on this particular point. Garrett says, "Spatial concepts apply logically when social behavior, for example, involves 'locomotion' or movement in space" whereas my expressed objective is to supply conceptual means for mathematical representation particularly of those social locomotions (like being initiated into a group or expelled from a group, etc.) and changes in social position which either do not at all involve physical locomotion or for which this physical locomotion is not essential psychologically.

The *Principles of Topological Psychology* by no means advocates the founding of a new psychological school. I state in the preface: "One of the main incentives of this book is to help develop a psychological language generally understandable and independent of schools" (2, p. viii). It presents conceptual means which have emerged out of and have been tested in a great number of experimental studies of my own students during more than a decade. What is more important, I believe, they can be shown to underlie many psychological procedures independent of any school. In the light of these facts, it seems to me only half true that "the value of this new 'ology' still lies in the future," particularly if Garrett says that "spatial analogies are not helpful when anger, aggressiveness, honesty, and the like, are to be represented." He overlooks the fact that, for instance, the experimental work of Dembo on anger (1), which I still consider to be one of the best in this field, has shown as far back as 1931 that topological and vector concepts are most useful means for accounting for the behavior and the dynamics of anger. Lippitt, White, and myself (4) have found these concepts most helpful in representing the constellations determining whether aggression occurs or does not occur in different social climates. In my mind, psychology, and particularly social psychology, will be bound to use field concepts the farther it progresses from statements of vague generalities to the task of prediction in concrete constellations.

One additional word in regard to a rather frequent mis-

understanding: It is not my opinion that historical problems should be eliminated in psychology (or that they are eliminated in physics). That is borne out, for instance, by the experiments of Dembo cited above. Instead of being satisfied with the momentary stimulus of a needle-prick as a source of emotion, as was the usual procedure of experimental studies of emotion at that time, Dembo used several hours, distributed over several weeks, in building up those psychological situations of anger which she wanted to study. A similar 'historical procedure' is characteristic of most of our experiments. Indeed, it is one of the main points of a field theory to emphasize that behavior cannot be explained without taking into account what Garrett correctly called the 'background.' However, this experimental and theoretical emphasis of the importance of the historical side of events does not mean that it is correct to treat questions about conditions (systematic problems) in the same way as questions about origin or fate (historic problems). Both are perfectly legitimate problems but their theoretical mixup is one of the main hindrances to quicker progress in such fields as learning and regression.

The reader will find a recent survey of the history of topological and vector psychology in Lewin (3). One of the main objectives is to make certain geometrical branches of mathematics as useful to psychology, particularly for handling problems concerning conditions, as is algebra, in the form of statistics useful at present.

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